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Psychological Review

EDITED BY
HERBERT S. LANGFELD
PRINCETON UNIVERSITY

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THE PSYCHOLOGICAL REVIEW

SYMPOSIUM: THE EGO AND THE LAW OF EFFECT

THE EGO AND THE LAW OF EFFECT[1]

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I

In a provocative paper (1), G. W. Allport has argued that psychology made a mistake in throwing overboard the 'ego' or 'self' along with the 'soul,' and that the time may have come when it is both possible and necessary to rehabilitate the first two of these concepts on a scientific rather than a theological basis. The eclipse of the self, he points out, became almost total when reflexology and the more mechanical versions of behaviorism chose to concentrate, for experimental purposes, on a segmental approach to behavior and thereby lost sight of the larger patterns and unifying features of the organism's activity. Allport cites a number of indications that not only psychoanalysis but several other schools of psychological inquiry are making an effort to rediscover the self, and he offers valuable suggestions as to the directions which this enterprise may take. In his treatment of the problem, however, he sometimes gives the impression that the 'ego' refers to something ultimate, unanalyzable and lawless. Though elsewhere (2, 3) he makes it clear that he rejects such notions as a substantial self and an ego 'instinct,' and that he holds the self to develop gradually under the nurture of experience and not by maturation alone, we are not given any clear principles to account for its genesis and transformations.

More particularly—and this is the section of his paper with which I am here concerned—Allport argues that the Law of Effect, at any rate in the form in which it is usually understood, is inadequate to deal with what he calls 'ego-involved' behavior. This is a charge that must be taken seriously. The last fifteen years have witnessed a widespread tendency to revive the Law of Effect, which had suffered from general disfavor during the 1920's, and to erect it into the principal, and even for some writers the sole, law of learning and acquired motivation. The thesis which I should like to present is threefold: (1) that Allport's criticisms are such as to force a reformulation or at least a clarification of the Law of Effect, but (2) that his evidence is such as to support rather than to refute the Law

[1] This paper was read, in a briefer form, at a Psychological Colloquium, in which O. H. Mowrer and G. W. Allport also participated, at Harvard University on December 12, 1945. The papers of Drs. Mowrer and Allport will follow Dr. Rice's paper. This paper by Dr. Rice represents part of the work done on a research grant from the American Philosophical Society.

when it is so reinterpreted, and (3) that the Law when thus clarified may supply us with precisely the principle that we need in order to account for some central characteristics of the ego's development.

Before taking up Allport's criticisms, I shall first indicate the present state of opinion with regard to the Law of Effect. In earlier formulations by Thorndike (17, 18), the Law of Effect stated that a connection between a stimulus-situation and a response tends to be strengthened by success or satisfaction or reward, and to be weakened by frustration or annoyance or punishment; the strengthening or weakening is proportional to the degree of the satisfaction or annoyance, and to the promptitude with which this follows upon the response. Later, as a result of many experiments both by himself and by others, Thorndike abandoned the negative clause, and held that (a) punishment may strengthen a connection, but when it does so the reinforcement is much less than in the case of reward, so that if an alternative rewarded response occurs it tends to replace the punished response, and (b) escape from punishment is itself rewarding, and tends to strengthen responses avoidant of punishment (19, 20, 21). In both cases the net tendency of punishment to weaken a connection follows from the positive clause, so that the negative is superfluous as well as inexact.

In its revised form, the Law of Effect has been shown by an impressive mass of experimental evidence to work well for many types of animal behavior and for simple human reactions where certain complicated interests, such as self-assertiveness, are not present. Many psychologists who once opposed the Law of Effect, including a number of leading behaviorists, are now coming around to respect it, believing it to be a more adequate principle of learning than the Law of Exercise or the Pavlovian and Watsonian principle of learning by simple conjunction and repetition (4, 14). The Law of Effect has been restated in an elaborate set of postulates by Hull in his doctrine of reinforcement by need-reduction (8). Freud at first offered the Pleasure Principle, roughly equivalent to the Law of Effect, as the sole law of motivation; he later added the Death Instinct, or repetition compulsion, which is in opposition to it (5). Many psychoanalysts, however, have refused to accept the Death Instinct as an original principle, and have tried to derive the death wish and the repetition compulsion from the Pleasure Principle (11, 12). Recently Mowrer and Ullman have supported the Law of Effect as the sole principle of learning, and have suggested ways in which the repetition compulsion may be derived from it (14). In his latest treatment of the subject (20), however, Thorndike himself accepts both the principle of repetition (Law of Exercise) and the principle of reward (Law of Effect) as independent laws, but he holds the latter to be much more important than the former.

The chief theoretical difficulty that remains on this score would seem to center about the status of punishment after the elimination of the negative clause. Thorndike's latest position implies that the occasions on which punishment directly strengthens a connection are taken account of by the Law of Exercise, so that it is repetition and not the accompanying punishment as such that reinforces a connection in cases where reward is absent. Mowrer and Ullman do not recognize the existence of such cases, but hold that all cases of apparent reinforcement by exercise are due to the indirect operation of reward. Miller and Dollard have upheld a similar position (13). Further evidence would seem to be re-

quired, however, on so difficult a question; by accepted neurological principles, it is hard to see how mere exercise or repetition, without satisfaction or need-reduction, could fail to have some effect on the associative neural tracts, even though this effect may be slighter than the 'retroflex' action of reward.[2] So it is perhaps safer to conclude that the repetition compulsion has not yet been absorbed entirely by the Law of Effect. One may hope that it will be possible to abolish the repetition compulsion as an original principle, so that we may adopt a more optimistic view of human nature, but the prudent course at present would be to suspend judgment.

In this discussion I am not concerned to try to resolve such difficulties, but I prefer to leave it an open question whether the Law of Effect can be taken as the sole principle of learning, or whether a Law of Exercise is also needed, perhaps together with still other principles. And I shall defer until later in the paper consideration of the weighty criticisms of the Law of Effect that have been advanced by the Gestalt school, since I believe that the Gestaltist charge that the Law has been tested chiefly by experiments on trial-and-error learning and rote learning, to the neglect of genuinely productive ('insight') thinking, may have been dismissed too cavalierly.

My primary purpose at this point is to consider some other objections to the Law of Effect that have been advanced by Allport in the paper which has been cited. To summarize these criticisms very briefly: Allport adduces some experiments by Hoppe (7) and Rosenzweig (16), and some observations of his own where successful execution of a response does not strengthen but weakens the tendency to repeat it. Children who successfully perform assigned tasks do not usually go back to do these tasks over, but prefer to tackle something else. A student who gets an A in a course does not repeat that course, but may take another in the same field. Repetition of successful acts is ordinarily a mark of infantile, deficient or pathological behavior rather than of intelligent adult activity. Allport concludes: "The relation between success and repetition, I suspect, is much closer in the case of non ego-involved behavior than in the case of ego-involved behavior. . . . Ego-involved tasks often involved changing goals and new responses. . . . In order to employ the Law of Effect with human learning we must view it as secondary to the principle of ego-involvement" (1, p. 468).

These objections are valid against a segmental or mechanical application of the Law of Effect which ignores the complexity of motives involved in most human activity, and the variability of our responses to similar situations and in pursuit of similar goals. The Law cannot be upheld if it means that success or satisfaction necessarily enhances the tendency for either or both (a) the specific response sequence to be repeated, and/or (b) the particular,

[2] In support of their contrary view, Miller and Dollard point to Adrian's evidence that "the sudden onset of a new stimulus produces at first a strong burst of impulses from the sense organ which rapidly diminishes in strength till a plateau of stimulation is reached" (13, p. 35, n.), and infer that it is the satisfaction produced by the diminution of stimulation, and not the annoying stimulation itself, that gives the reinforcement. This explanation would seem to overlook: (a) the possibility in certain cases that the stimulation may not fall to a plateau where it is rewarding rather than punishing, and (b) those cases where the stimulation in question is replaced centrally by another stimulus before the level of excitation has had time to drop below the satisfaction-annoyance threshold—Adrian's experiments deal with an isolated source of stimulation to a sensory nerve and therefore do not take such a situation into account.

or even the specific, goal object to be chosen again. Repetition of *both* these elements occurs chiefly in very young children, in mental defectives and in compulsion neurotics. With normal adults, both are repeated mainly in the case of routine habits and recurrent appetitive drives: like many of my compatriots, I nearly always have orange juice, bacon, scrambled eggs and coffee for breakfast, when I can get them, and I consume them with a fairly stereotyped sequence of acts. Even here repetition occurs only in the absence of a problem-solving interest (the desire to negotiate a new kind of dish in a strange country), or ego-involvement (the wish to impress one's distinguished hosts by showing a capacity to master fried kippers), or the quasi-aesthetic demand for variety (my aesthetic tendencies are usually not awake by breakfast time).

When normal adults repeat one of these two elements—the response as a whole or the choice of the particular goal object—after success, it is usually with a variation in the other. If one puts the same puzzle together twice (*i.e.*, repeats the choice of a goal object), it is usually to do so in a different way (*i.e.*, to master a new response sequence). If a college boy repeats the same 'line' (response sequence) it is with a different goal object (to impress a new girl). Successful use of a response sequence in attainment of one goal may nevertheless encourage its repetition in pursuit of a similar but not identical goal; and successful pursuit of a goal may lead to adoption of a similar but not identical goal in the future, and to use of a similar response sequence. Or, we may vary both the response and the goal, so far as the details go, while repeating with variations the same general *kind* of response and selecting the same *kind* of goal that brought us success in the past.

When we change both the specific response sequence and the particular goal, it may still be true that *something* about the activity is repeated, and that satisfaction from this aspect of the activity is the ground of the repetition. We may have several different levels of specificity and generality here. Varied, but partially similar, response sequences and goals are involved in solving different problems in algebra; success with one algebraic problem may encourage us to tackle another algebraic problem. Or, success in algebra may encourage us to take up trigonometry: both have the common property of setting mathematical problems.

These examples suggest that it is not the response sequence as a whole, or even any stereotyped segment of it, that necessarily tends to be repeated, but only certain general features of it, such as the 'interest' that is involved. The only possibility of saving the Law of Effect, then, would seem to consist in finding *what* is reinforced by success or satisfaction, and therefore *what* it is that tends to be repeated. This implies that we should either replace 'response' by some other concept, such as 'interest,' in the statement of the Law, or else try to find some *aspect* or *feature* of the response, rather than the response as a whole, which is reinforced by satisfaction; in the latter case, the feature selected must include for human activity, and possibly for some animal behavior, the core of what is intended by the vague term 'interest.'

II

The examples cited by Allport against the Law of Effect always have one or more aspects in common, and these aspects are more general than either the response mechanism or the class of goal object. The individual cases share at least one of these characteristics: they are examples of *problem-solving*, or

they involve *satisfaction of the ego*, or they embody the *interest in novelty*. If the Law of Effect can be upheld, it must take into account the shifting levels of generality and prepotency that may be involved in motivation. It follows that the satisfactions and dissatisfactions in question must include those attaching to the interests in problem-solving or self-assertion or variety.

Can we then establish a correlation between satisfaction and a tendency to repeat types of acts containing these characteristics?

1. *Problem-solving.* By definition, this excludes repetition of both the same goal and the same response sequence. When both of these are repeated, there is no 'problem'—except in the special case where we 'want to see if we can do it again,' and here whether we can do it again is itself the problem to be solved, and a different stimulus-situation from the original one. All the cases Allport and Hoppe have stressed are cases where the primary *interest* is in problem-solving, or in ego-gratification dependent upon problem-solving. Where there is no such interest, and where the interest in novelty is not involved—as in the case of my breakfast—then a fairly simple application of the Law of Effect is possible. The Law applies only to the total nexus of interests, and its use must take into account their relative strengths and their subordination.

I cannot get satisfaction from problem-solving unless there is a problem. So it is unfair to cite against the Law of Effect cases, like working puzzles a second time, in which the primary motivation would be problem-solving if such motivation existed, but in which by hypothesis it cannot exist. A puzzle no longer puzzles me when I have learned how to solve it. The stimulus-situation has changed.

The question would seem to be, then: Does satisfaction of the interest in problem-solving tend to reinforce that interest, in the absence of conflicting interests, and does frustration of it tend to replace the interest by some other? Here again we must discuss the question on at least two levels of generosity.

(a) Does success in solving a given *kind* of problem, other things being equal, reinforce the interest in that kind of problem? None of the evidence cited refutes the hypothesis that this is the case: the apparent exceptions involve the intervention of some stronger interest. In general, with the people who devote themselves to mathematical problems, or chess problems, or psychological problems, or amatory problems (I am thinking here of the Don Juans and Casanovas, who have a connoisseur's interest in these problems for their own sake), success in the long run tends to reinforce the interest, and failure to lead them to devote themselves to some other interest in which they have achieved a greater measure of satisfaction. Either the individual goal or the specific response always varies, but the interest when stimulated tends to be repeated with success. When we persist in such activities despite repeated failure, another type of interest to which the one in question is auxiliary can usually be found. The persistent desire to be an engineer may keep a student at mathematics despite repeated failures; but unless he has some partial success in solving some of his engineering tasks, or unless some more embracing ego-motive is involved, he will tend eventually to seek some other vocation, if there is another skill in which he attains proficiency. And his persistence despite failure may be explained in part by the fact that 'satisfaction' and 'success' are only roughly equivalent concepts: the individual may get some satisfaction out of trying, even though he fails. It is a truism

that some people are so constituted by their training that to fail at a difficult task gives more satisfaction to the ego than to succeed at an easy one.

We do find people who are always passing on from one *kind* of problem to another, even though they are successful with all kinds. But here also the interest in the specific kind of problem seems subordinate to some ego-motive, or to some more general and powerful interest in novelty, both of which require special treatment.

(b) Is the most general of all interests in problem-solving—problem-solving as such, irrespective of the *kind* of problem—itself subject to the Law of Effect? I believe that everyday experience bears this out, still making allowance for more embracing motives. Except for some mental defectives, there is perhaps no one who has no interest in problem-solving, either for its own sake or as a means to the satisfaction of some specific interest. But there are considerable variations in the intensity of the interest, and in whether problem-solving is undertaken as a means or as an end. We all know people who are not eager to face problems, much less to seek them out, and who put off their solution as long as possible. When such people do solve problems, it is rarely the intellectual or dramatic interest in problem-solving as such that gives them satisfaction, but the ulterior interests which generated the particular problem. But repeated success in solving problems that are forced upon him may make such a person adventurous and may lead him to seek out problems. The best educational practice seems to assume such a hope. The person who is incompetent at reaching solutions, on the other hand, will 'take things as they come' and rely upon more passive or receptive interests. So it would seem that the general interest in problem-solving is, like other interests, enhanced by satisfaction and stifled by frustration, except where organic needs or ego-involvement complicate the picture.

2. Allport makes an exception to the Law of Effect in the case of *ego-involvement*. He writes: "The relation between success and repetition, I suspect, is much closer in the case of non ego-involved behavior than in the case of ego-involved behavior" (1, p. 468). Even here, however, he might accept the Law of Effect in a revised formulation. What I am suggesting is that ego-involved behavior is on a different level of generality of interest, and that the Law of Effect may apply here too unless we try to restrict it to a lower level.

We must avoid two extremes—conceiving the self as a bundle of separate and unrelated reflexes or even unrelated interests, and conceiving it as a mystical unity not subject to analysis and genetic explanation.

The ego is a system of interests; 'system' implies unity, and 'interests,' being plural, implies multiplicity. The unity is presumably supplied by such factors as the hierarchy of dominant and subordinate interests, the level of aspiration, and the 'style of life,' with attendant attitudes such as self-respect, shame and the like, evoked by satisfaction or frustration of the interest-cluster in question. Very likely the unity of the self is to be conceived, like any Gestalt, as an emergent factor, and therefore one that is capable of becoming functionally autonomous. But this does not mean that we cannot go behind it to study its genesis, to find principles governing its changes, and to analyze the various conditions that regularly give rise to its configuration. If not the Law of Effect, then some other law or set of laws specifying the conditions of reinforcement must hold of the ego's development—this would be the assumption of a scientific psychology.

The Law of Effect has not been shown to be inapplicable to the development of the ego as so conceived. The most sustained attention to the problem of defining the ego and tracing its genesis has been found in the work of the psychoanalysts, Freudians and post-Freudians, who have sought the origin of 'basic personality structure' in the relationships involving affection and authority between parent and child (9, 10). Though the depth psychologists have given little explicit mention to the Law of Effect in dealing with such matters, their stress on the importance of satisfactions and frustrations, rewards and punishments, in character formation suggests that the development of the ego itself may be subject to Thorndike's Law. Level of aspiration and ego-level are surely affected in any given society or any given life by rewards and punishments. And these influence not only the level but also the specific content of the ego-cluster. A 'high' level of aspiration to monetary success is fostered in this country by advertising, the movies and public opinion; it seems to be discouraged among the Marquesans and Zuñis. A high level of meditative absorption is rewarded among the Brahmans. A high level of literary aspiration is fostered by the *cénacles*. A high level of honesty, financial or intellectual, may be encouraged by one's family training. In large part, at least, the level of aspiration here, and its specific direction, do not seem to be ontogenetically emergent from simpler interests of the individual, but to be implanted directly as a unit trait by the social pattern, through society's ability to award and withhold satisfactions and dissatisfactions.

What can cause an individual to persist in a 'style of life,' or the pursuit of it? Observation suggests that he does so because he has found satisfaction, actual or imaginative, from that style of life, or because it promises to integrate the scattered elements of past satisfactions and lessen past dissatisfactions, or because social rewards and punishments promote its adoption. But all these are operations of the Law of Effect.

Since the self is a system of interests, self-assertiveness in general is extinguished only when these interests are extinguished, and when no other system of interests has supervened to take their place. Self-assertiveness is not downed easily, and takes subtle revenges when thwarted, as the psychoanalysts, and before them the novelists, have shown. We should expect this from the Law of Effect itself, because the interests which go to make up the ego-cluster are those which supply continuity to the individual's life pattern, those which have constituted the area of his conflicts, those to which he has given most thought, and therefore those which have been reinforced most often in anticipation and retrospect, even if not by the actual achievement. But self-assertiveness can be deadened, and even to a large extent stamped out. We occasionally find people in whom it has been largely extinguished, and who go through life responding mechanically to external demands and executing their minimum organic drives. In such people the interests which constitute the ego-cluster, and their successive substitutes, have been frustrated so completely that they have been very nearly stamped out: their persistence is to be accounted for by the inability to find any rewarded responses to take their place. Such persons are usually found among slaves—legal or economic—the extreme victims of war, and brow-beaten husbands and wives.

3. Another interest on something like the same level of generality as problem-solving and ego-involvement is the

interest in *novelty or variety*. This interest may arise through dissatisfaction with routine, or be implanted by a frivolous society or an aspiration for intense aesthetic rewards from life. But when it once appears, it seems to be subject to the Law of Effect. The individual to whom novelty brings satisfaction will, other things being equal, seek more of it; the individual whom it bewilders will return to use-and-wont.

The upshot of all this is that the validity of the Law depends on *what* is conceived to be satisfied or thwarted, and therefore reinforced or extinguished. Only because many proponents of the Law, influenced by the cruder forms of reflexology, have focused on response mechanisms and goals rather than interests, and have ignored interests of high generality, has their proof of the Law been so inadequate.

Let us apply this to the case, cited by Allport against the Law, of the student who does not repeat a course even though he has obtained great satisfaction from receiving an A in it. He does not repeat the response involved in registering for the course and attending class, because this would interfere with interests motivating the original act, such as those in obtaining credits (the registrar has a say here), in problem-solving, in novelty and in proving to himself that he can master a new subject. But it may be precisely the past satisfaction of these interests that will determine him to choose a *new* course rather than to repeat the old, and he may choose the new course for its probable capacity to gratify those interests further. So that the example seems to substantiate the Law of Effect rather than to refute it, when completely analyzed.

The Law of Effect, so applied, is also compatible with what Allport has said about the 'functional autonomy of motives' (3); indeed it, or some principle with a similar scope, is needed to explain how some motives become autonomous and some do not. His discussion of the subject leaves this point in the air. Let us take Allport's case of the person whose interest in literature was first subservient to an interest in sex but persists despite the satiation or extinction of that interest, or independently of it. Here we have simply a case of mixed motives or complexity of interests. If the individual has received satisfaction from the non-erotic elements of poetry, whatever his original motives in reading it, he will tend to repeat the activity in cases where sexual interest is not aroused. But would he retain the literary interest independently of the sexual interest if the former had given him no satisfaction, or if it has not become yoked to some new interest? It is precisely when the originally subordinate interest gives intrinsic satisfaction that it becomes autonomous.

This separation of interests originally entwined may take place at either a reflective or a sub-reflective level. Generally it is more effective, *i.e.*, satisfaction is maximized, when we learn to analyze and disentangle our interests, than when we leave it to the fortuitous play of external stimulation alone. But our tendency to be reflective seems itself to be subject to the Law of Effect; the tendency is reinforced only in those people in whom it is successful, *i.e.*, those who have learned to think straight. Those who think crookedly usually get themselves into trouble, and would do better to rely on intuition, or custom, or the guidance of a spiritual adviser—this is, in fact, what in the long run they will do.

These remarks fit in with Thorndike's own later statements about the Law of Effect (21), though he has not given a precise reformulation of it. There he has tended to treat reinforcement in

terms of 'wants,' 'interests' and 'attitudes,' as well as in terms of stimulus-response connections; and where he does refer to responses he says that reinforcement can attach to any 'part or feature' of them, that the response may consist of an idea, a mood, a liking, a craving, as well as a motor act, and that the reinforcement operates upon the 'relevant' or 'belonging' aspects of the response. And for human motivation, Thorndike has stressed the 'confirming reaction' or 'O. K. reaction' as the kind of reward that is most potent. The confirming reaction is strongest when it is a gratification of the ego by way of self-esteem or a gratification of the super-ego by way of 'conscience.'

In such cases, the confirming reaction is something 'internal' to the individual, and not always, or primarily, an externally administered reward. The external reward operates, anyhow, only when it evokes an affective response, which may or may not issue immediately in overt behavior. We may have here the foundation for a restatement, in scientific terms, of the age-old ethical insight that it is what goes on inside a man that is decisive; and we may be able to see more clearly why a person in whom such tendencies to self-approbation have been established may persist, in his heroism or folly, with a course of action that will bring him physical hardship, social punishment and even death. But such considerations require us to take into account complexities of human motivation that have been hitherto ignored by most of the experimental work on the Law of Effect.

III

So far we have seen that there is good reason to hold that *something* about the organism's reaction tendencies is reinforced by satisfaction. For certain types of human activity, we have found it useful to treat this something in terms of 'interests.' It is now time to analyze this vague concept more carefully, and to try to overcome the disjunction between human and animal learning, or between ego-involved and non ego-involved motivation, that the foregoing discussion has seemed to assume, so that the Law of Effect may be conceived in such a way as to comprehend both the simpler and the more complicated types of behavior. We need particularly to understand why satisfaction on one occasion reinforces a stereotyped response sequence, and on other occasions reinforces a general interest which may require changing goals and varied responses.

I have suggested that it may be possible to interpret reinforcement in terms of 'responses,' if the reinforcement is held to attach selectively to some 'parts or features' of the response, including those features of it which constitute the core of an 'interest.' Whether it is helpful, as Thorndike has continued to do, to retain the notion of a *connection* between a stimulus-situation and a response as the basic notion in the statement of the Law is another question. The initiating stimuli in the case of such a reaction tendency as an interest may vary considerably, and these stimuli as a whole may be novel or unique or 'accidental.' Efforts have been made to deal with this difficulty in terms of 'stimulus generalization' and 'response generalization' (8). But the question is so complicated that it would be wise to restrict the present inquiry to finding some feature of the reaction that is reinforced by satisfaction, whatever may be the stimulus which evokes it, and even though it may be held to supply, in some cases, its own recurrent stimulation.

In accordance with a recent paper by Mowrer and Ullman (14), I should like to suggest that the Law of Effect should

take into account the human being's capacity for symbolization. The confirming reaction for humans, and to some small extent for the higher animals, can attach directly to symbols, rather than to those features of the act that are symbolized, or to those features that are not sufficiently 'relevant' to be taken account of by symbolizing them.

That core of the act which constitutes the 'interest' is the feature of it which is most likely to be symbolized and repeatedly confirmed through approval of its symbol. It is therefore impossible to define the interest adequately for all cases in terms of any one feature of the act, such as the goal-object, or the terminal consummatory reaction, or the preliminary response sequences. In some activities, *e.g.*, the economic, we are primarily interested in the goal-object, money or the things that money can buy. In amatory activity, we may be interested in the preparatory features of the act as well as the consummation. When we are playing a game for the sport, and not primarily to win, our chief interest is in the performance of certain skills rather than in the attainment of the ultimate goal. In listening to music, we are especially interested in the pattern of sounds, that is to say the perceptual elements of the situation, together with our immediate affective responses to them. In these cases the interest is defined by various features of the act, but always by those to which satisfaction chiefly attaches, and it is consequently these several phases of the act that are reinforced.

Rational or reflective activity requires singling out, by responding to their symbols, such general characteristics of the act in advance of its performance; and we speak of the crucial features as constituting the 'purpose.' Rational behavior is distinguished from non-rational by analysis and symbolization of the purpose, and by the implementation of this through symbolization of the means of achieving it. What is most reinforced by success is the purpose rather than the intention or the specific response sequence as a whole or even any particular temporal slice of the latter. The purpose is that aspect of the act for the sake of which it is done, that to which satisfaction has attached in the past or to which it attaches by way of anticipation; the intention includes the projected means as well as the end. There is no necessary tendency to repeat the instrumental or auxiliary responses after success unless they have also acquired consummatory or end value, that is, have given satisfaction autonomously and are recognized to have done so, or unless their symbols have been so conjoined with the symbol of the purpose that the satisfaction 'spreads' to them too. We show great flexibility in adapting our means to our ends, and we may change our goal-objects freely provided that the pursuit of them is expected to provide, somewhere in the process, those qualities or structures of the act to which satisfaction has adhered.

How then, in accordance with the Law of Effect, does reinforcement occur in the cases of reflective and non-reflective activity respectively? Two concepts may help us here: (a) Thorndike's notion of the 'snow ball' effect of the confirming reaction (21), and (b) that sub-principle of the Law of Effect known as the 'gradient of reinforcement' (8, 15, 19).

In animals capable of symbolization, an act that receives approval does not give us just a single satisfaction upon the completion of it. It gives us satisfaction by anticipation and in retrospect. Our symbol of the purpose *intervenes* between the stimulus and the overt response, and we O. K. it before

we respond. There may be many such anticipatory reinforcements while we are planning the details of the act, or waiting for an opportunity to perform it. As we are carrying out the successive phases of the act, we keep the symbolized purpose in mind, mull it over, and confirm it repeatedly; unswerving concentration on the purpose, as it begets auxiliary goals, is necessary to keep us going. Successful execution of the act gives the purpose further reinforcement; then after the act we may reflect upon it and obtain repeated satisfaction in retrospect. So the reinforcement 'snow balls,' or multiplies itself.

A dynamic connection must, of course, be established between the symbol of the purpose and the responses necessary to execute it, or else the reinforcement remains at the level of fantasy and wish fulfillment. But this dynamic connection seems to spring from acquired habits of implementing a given kind of symbols by action, and not to require the previous execution of a specific response to the particular configuration of symbols involved in any given act. Otherwise, novel situations would never be met intelligently.

The gradient of reinforcement—according to which reinforcement is an inverse function of the time elapsed between a given feature of the act and the confirming reaction—is obviously operative along with the snow ball effect. In all the above phases of the reflective act, the confirming reaction is simultaneous with, or follows immediately upon, the symbol of the purpose, and so attaches more directly to it than to the other features of the act, which in anticipation and retrospect may not be constantly present at all. So it is the purpose, or interest, that is reinforced more strongly than the details of the response, and the crucial instrumental acts are reinforced next most strongly. It is this attachment of the confirming reaction to the symbolized purpose, and to the crucial means, which constitutes the 'relevance' or 'belongingness' in the rational act which Thorndike recognized but was unable to explain adequately, and which may help us to dispel some of the haze surrounding these concepts in the writings of the Gestaltists.

In the case of ego-involved behavior, the purpose may be to exceed our previous performances, or to prove our versatility by mastering a new kind of response. Such interests preclude the exact repetition of a previously successful response, but this does not imply that the general purpose itself is not reinforced by satisfaction.

In non-rational or non-symbolic activity, where effective learning occurs, reinforcement attaches—also according to the principle of the gradient—most strongly to the immediately preceding phase of the response, to the 'last open path' (6, 15, 23), *i.e.*, to the consummatory reaction and the penultimate preparatory response. This accounts for the fact that in animal learning, as well as in human, it is only certain selected features of the response that are reinforced, and that there is consequently a great deal of variability in the means employed to reach a given type of goal. In the case of irrational or non-integrative learning, the confirming reaction attaches to some 'accidental' (non-causal) feature of the situation, or to faultily devised symbols (14). But in both sub-rational and irrational behavior, as in effective symbolic learning, the feature of the act which immediately precedes the repeated satisfactions is most strengthened. It follows that, when allowance is made for the differences in the situation created by the presence or absence of symbols, and by the correctness or incorrectness of the symbols,

all these types of learning alike may be brought under the Law of Effect.

IV

In the above, I have not attempted anything like a 'proof' of the Law of Effect, nor even a precise and detailed reformulation of it. These would require much more experimental and observational evidence than has been adduced, and attention to different kinds of evidence from those that are usually cited for or against the Law. What I have tried to further is analysis of the problem, and in particular a way of applying the Law of Effect to ego-involved and rational behavior in such a manner as to meet Allport's objections. In conclusion, I should like to suggest some further considerations that must be taken into account before a proper application of the Law to learning and motivation becomes possible.

Allport and others have urged as an objection to the Law that the driving power in an immediately operating motive comes from a dissatisfaction rather than from a satisfaction. Only the dissatisfied person, the person who is deprived of his goal at least temporarily, strives to achieve it.

Most proponents of the Law, I believe, would agree with this point, but would hold that it is an objection to the Law only if we look to the Law of Effect to do what it is neither suited nor intended to do. It is not a principle to describe the dynamism of immediate motivation. Its scope is the acquisition and long-run reinforcement of motives, or the influence of the past on the present. It constitutes, as Troland called it, a 'hedonism of the past' (22). For an understanding of the immediate motivational situation, we must examine the play of forces operating in the present, as well as the influences controlling these from past experience.

A complete analysis of the complex state of affairs with regard to motivation would need to include the following facts. A given interest, unless interfered with by other interests, tends to be fixated by satisfaction. When the goal object has been attained, the interest seeks to hold on to it, to sustain the satisfaction as long as that persists (in the absence of inhibiting stimulation). Before the goal object has been achieved, in order that the interest may operate at all, there must be a gap between its arousal and its satisfaction. The dynamic character of an act in progress, in the latter case, derives from an immediate dissatisfaction, from an interest which in the particular case has not achieved its goal. But the interest as an enduring disposition, a general tendency of the organism which outlasts its particular activations, will be reinforced by repeated satisfaction.

The Law of Effect, then, is a law of retention of interests or dispositions; it does not deal with the mechanism of the particular striving, except in so far as that is determined by past fixation of interest as one of its conditions. The tendency of a particular striving to persist despite repeated frustrations is limited by the relative strength of the interest, which in turn is dependent upon its reinforcement by past satisfactions where it is autonomous, and upon the strength of other interests where it is subservient.

It may be along these lines that we should look for a reconciliation between the proponents of the Law of Effect and the Gestaltists, who have generally been its opponents. The Gestalt school have concerned themselves scarcely at all with the effect of the past upon present motivation, but almost entirely with catching the flying vectors of immediate motivation upon the wing. And they have devoted themselves especially, in learning theory, to the psychology of discovery, to the complex and creative types of learning

where a problem is solved for the first time and where intelligent manipulation of symbols replaces blind trial-and-error and mere repetitive drill. In particular, they have tried to describe the way in which the elements in a situation characterized by dissatisfaction regroup themselves in order to provide satisfaction or closure.

I see no reason why the Law of Effect should be expected to provide the complete answer to such problems, nor, on the other hand, why it should be *prima facie* irrelevant to them or incompatible with the solutions offered by the Gestaltists themselves. Where we have a novel problem to be solved, no repetition of a past act, or stereotyped combination of features of it, will be adequate to give us the type of learning called for. But it still remains possible that the habits or tendencies that contribute to 'insight' are themselves fostered by past experience, and are reinforced by success or satisfaction. A growing, if somewhat grudging, recognition of this may be bound in some of the writings of the Gestaltists themselves, and particularly in the recent book of the late Max Wertheimer:

"In short, the role of past experience is of high importance, but what matters is *what* one has gained from experience—blind, un-understood connections, or insight into structural inner relatedness. What matters is how and what one recalls, how one applies what is recalled, whether blindly, in a piecemeal way, or in accordance with the structural requirements of the situation" (24, p. 62).

"*Learning on the basis of success* may mean that an action is singled out because of the success that follows the action only factually, but is not understood; or it may mean that, in learning, a subject grasps why just this kind of action leads to just this effect for intrinsic structural reasons. It is the latter form of 'learning by success' that enables the subject to vary his action in a structurally sensible way when the situation is no longer the same" (24, p. 206).

In Section III above, the attempt has been made to suggest, through a discussion of the part that symbols play in the process, how reinforcement may attach to the 'relevant' or 'required' features of the act, so that some of the mystery that has accompanied the concept of relevance in the writings of the Gestaltists may be dispelled by the Law of Effect. The implication is that both the Gestaltist description of immediate motivation and the treatment of long-term reinforcement by a revised Law of Effect are needed to supplement and buttress each other.

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THE LAW OF EFFECT AND EGO PSYCHOLOGY[1]

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In his paper, 'The ego and the law of effect,' Professor Rice[2] has performed a singular service. With the detachment of a true philosopher and an incisively critical and discerning eye, he has surveyed some of the most controversial areas in the whole field of psychology and has produced a synthesis which we psychologists, with our intensely partisan and often narrow preoccupations, can read—and re-read—with profit. As I have studied Dr. Rice's paper, it has been with a growing appreciation of its thoughtful subtleties as well as its systematic coherence and soundness.

Rice alludes to the challenging paper by Professor G. W. Allport, 'The ego in contemporary psychology' (3), in which the author calls for a reconsideration of some of the phenomena which used to be subsumed under the terms 'ego' and 'self' but which, for several decades now, have been taken seriously only by the psychoanalysts and certain Gestalt theorists. Elsewhere (23, 24, 26) I have advanced parallel reasons for supposing that the time has come when 'scientific,' or 'objective' psychology can profitably undertake the investigation of a number of long-neglected problems in this area, and that until this is done we shall have neither a comprehensive, a unified, nor a maximally useful science of human action and experience. I am therefore in complete accord with Rice's appraisal of the importance of Allport's paper, and it is the fact that Rice so successfully reconciles Allport's position with the systematic point of view with which I am most familiar and sympathetic that makes the former's paper, in my judgment, so uniquely valuable.

I shall first discuss some of the major problems which are suggested by Allport's paper and then speak about the particular resolutions of these problems which Rice has proposed.

I

Professor Allport has been a brilliant and unremitting critic of the Law of Effect. He has probed its soft spots and revealed its shortcomings perhaps more effectively than any other writer. But while acknowledging the timeliness and cogency of many of his strictures, one may variously interpret their implications. Allport himself has been inclined to replace the Law of Effect with such concepts as ego-involvement and functional autonomy—concepts which, if not contrary to the Law of Effect, are at least assumed to be independent of it. I prefer, with Professor Rice, to think that although the Law of Effect has not always been aptly formulated, it nevertheless provides us with the firmest foundation on which to develop a truly adequate and comprehensive psychological theory.

Without attempting to be in any way thoroughgoing, let me cite a few of the reasons why I take this position.

1. The expression, 'The ego,' itself raises problems. It suggests, for example, that 'the ego' is a thing, a substance, a structure (3, p. 474). Neither *post-mortem* dissection nor vivisection has ever yielded such a structure. Obviously, *the ego* must refer to a process or processes. I should therefore prefer

[1] This paper, in a more abbreviated form, was prepared for and read at a symposium held at Harvard, on December 12, 1945.

[2] See preceding article in this number of the REVIEW.

to see the term used only as an adjective, to denote, for example, ego processes and non-ego processes.

2. Another implication of the term *the ego* seems to be that one either has an ego, a more or less full-blown one, or one does not have one at all. Allport explicitly states that small children do not have egos and that this part of the total person is an outcome of a particular type of development. But when, exactly, may a child be said to have acquired 'an ego'? And what about animals? Do they have 'egos'? If some do and some do not, what is the principle of differentiation? Again it seems to me desirable to speak of ego processes rather than egos. By so doing one is in a much better position to set up operational criteria for determining the precise extent to which such processes may be said to be in operation in any given organism and at any given stage of development. My own understanding of what ego processes are suggests that they are gradually elaborated both as the human child develops and as one ascends the phylogenetic scale.

3. Allport seems to equate 'ego' and 'self.' This, I think, is confusing. The self, or person, includes the total living organism. Thus, in common and accepted usage, if one cuts one's finger, one says, "I hurt myself." But one would hardly say, "I hurt my ego." This, along with the other difficulties mentioned above, are avoided if one thinks of ego, not as a *thing*, but as a process or processes.

4. The second term that strikes one in Allport's paper is 'ego-involvement.' Almost without exception, one can substitute the common term 'interest' for 'ego-involvement' in this paper without materially changing either the reader's impression or, I suspect, the author's intention. The term 'interest' is a very common one in educational literature (34), and its equivalent, 'cathexis,' appears frequently in psychoanalytic writings (7). I question whether either of these terms means anything more than *emotional arousal*, either appetitive or affective. Appetitive emotions occur when one anticipates a satisfaction or consummation of some kind; the affects, notably fear and anger, appear when one anticipates a dissatisfaction of some kind. We know a good deal about both the genesis and nature of the emotions, and I see no advantage in separating ourselves from this knowledge by speaking of ego-involvement instead of emotional arousal.

5. The substitution of emotional arousal for ego-involvement has a number of clarifying consequences. Let me instance one. In his paper, Allport says: "Mental health and happiness, it seems, [do] not depend upon the satisfaction of *this* or *that* drive; [they depend] rather upon the *person* finding *some* area of success somewhere. The *ego* must be satisfied, not the hunger drive, nor the sex drive, nor the maternal drive, however temporarily insistent these segmental tensions may be" (3, p. 466). Note that the author does not include, for example, the *fear drive* in this connection. Can this be because of an implicit recognition that so-called ego satisfaction is actually emotional satisfaction? It is certainly true that a person can be hungry, thirsty, cold, tired, or sexually deprived and—if not *too* hungry, thirsty, cold, tired, or deprived sexually—may still be "mentally healthy and happy." But one can hardly be chronically fearful or hopeless and be mentally healthy and happy. The fulfillment of hopes and the reduction of fears are, I submit, the essence of ego satisfaction and are not, in principle, different from the satisfaction of the so-called primary, or biologically given, drives.

6. In the foregoing paragraphs I have

obviously been trying to show that we do not need a whole new set of terms and concepts to understand and explain the motivational and consummatory processes which occur in the case of 'ego-involved behavior,' as contrasted with ordinary behavior caused by the familiar drive mechanisms. Allport agrees that the Law of Effect works well enough in the latter case, and I submit that it also works very well in the former case.

But if we stopped here, I am sure no one would be satisfied, least of all myself. Allow me, therefore, to make my own principal criticism of the Law of Effect and see if this does not lead us more nearly to agreement.

The very expression, the Law of Effect, implies that actions have *only one* consequence, *only one* effect. It is recognized that this effect may be either rewarding or punishing, pleasurable or painful. But in any given instance the effect is singular. By contrast, I submit that if we are going to have a psychological theory that accords with common experience, we ought to speak of the *Law of Effects*. In ordinary life, actions more commonly than not have *multiple* consequence, *multiple* effects, some rewarding, some punishing; some immediate and some remote. Psychoanalysis has explicitly recognized the admixture of pleasant and unpleasant consequences following any given action and has subsumed this fact under the term *ambivalence*. Psychoanalysis has also recognized that consequences may be unevenly distributed in time, some being relatively immediate and others more or less remote, and it has subsumed this fact under the distinction between the *pleasure principle* and the *reality principle*. (6).

7. The Law of Effect, as traditionally formulated, is an abstraction, or induction, from observations made in highly simplified laboratory situations in which the action of a living organism has a consequence, or effect, which is at one and the same time *unitary* and *immediate*. If a living organism performs an action which is solely and immediately satisfying, this action will (if the organism is 'naive') be repeated with increasing promptness when the same problem-situation recurs. Similarly if a living organism performs an action which is solely and immediately punishing, this action will tend to be inhibited when the same problem-situation to which it originally occurred recurs.

What the Law of Effect *does not* tell us is what living organisms will learn and do if they perform actions which are followed by consequences which are (a) both rewarding and punishing and/or (b) both immediate and remote. The investigation of these problems takes us into dynamic or, if you will, ego psychology. The processes by which consequences which are both rewarding and punishing are weighed and balanced and by which consequences which are both immediate and remote are integrated—these processes are, in my judgment, prominent among those which we may call the ego processes.

With this I believe Allport fundamentally agrees, for he explicitly states that the ego is the "conflict-region" of the total personality and that it is "customarily occupied with the future" (3, p. 474). Those processes by which conflicts are resolved and by which the future is brought forward psychologically and integrated with the present can be called "ego processes" quite appropriately (24).

8. The *mechanisms* by which these processes are mediated are, of course, another story. These mechanisms importantly involve that type of behavior which we commonly refer to as symbolic, and I suggest that the study of so-called symbolic behavior, especially in small children and animals, on a very

much intensified scale, is currently needed most urgently (24, 25). But I suspect that further research will show that symbolic behavior is itself learned and performed under substantially the same conditions as implied by the Law of Effect in connection with non-symbolic behavior.

By admitting the phenomenon of *conflict* and the element of *time* into our systematic psychological theorizing, we are in a position to reconsider such concepts as reasoning, willing, and thinking, which, for the past thirty years, have been in general disfavor. However, if and as we now turn to reconsider them, it will be from a new and more secure standpoint (24).

I fully agree with Allport that the post-war period is likely to be one in which psychology makes momentous strides toward overcoming the narrowness and limitations which it imposed upon itself in its adolescent revolt against philosophy. It is an encouraging sign that a person such as Dr. Rice, a philosopher concerned with the problems of value, finds that psychology already has sufficient maturity to be of some assistance to him in his work; and I take it as an equally good omen that an increasing number of psychologists are becoming less afraid of losing their scientific chastity than they once were by having contact and being concerned with problems of ethics, semantics, value, and other fields which formally fell within philosophy.

9. In attempting to build a systematic theory of behavior which will extend from the most simple to the most complex behavior of living organisms, one encounters a number of perplexities and complexities, none of which, however, seems insurmountable. Let me mention but two.

If we agree that satisfaction is the cement that makes learning stick, whether this learning be mediated by symbolic or non-symbolic processes, we are faced by the problem of defining 'satisfaction.' Most writers agree that satisfaction ought to be equivalent to pleasure and dissatisfaction equivalent to pain. But, then, what do we mean by 'pleasure'? One view is that we can equate pleasure to drive—reduction and pain to drive or drive-increase. Some writers object that tension-increase may, in some instances, be pleasant. Such instances are, I believe, deceptive and upon more thorough analysis turn out to be consistent with the thesis that satisfaction, pleasure, and drive-reduction are strictly equivalent (24).

If, however, one wishes to temporize a bit, one may say that satisfaction is the subjective consequence of solving a problem. I see no objection to this statement, and if it is more satisfactory to others, I am content to use it instead.

10. The second problem which I wish to mention in this connection is that organisms in which the symbolic (or 'ego') processes are poorly developed seem to exemplify clearly enough the 'hedonism' implied by the Law of Effect but that organisms in which the symbolic processes are *well* developed often seem to transcend and even defy hedonism. Normal, personally mature human beings manifest sentiments of honor and obligation which seem to rise above all considerations of pleasure, satisfaction, or reward. We work, we save, we sacrifice. We try, as best we can, to be virtuous, and the common element in all virtue is renunciation. To put the problem in its most paradoxical form, it seems that human development requires that we learn *not* to learn under at least some circumstances or, one may say, we must come to feel rewarded because of not allowing ourselves to be rewarded. We must, in a word, become ethical beings and this seems to imply a repudiation of hedonism and pleasure-seeking.

I have previously attempted to show the desirability of distinguishing between three frames of reference in behavior theory: the adaptational, or biological; the adjustive, or hedonistic; and the integrative, or ethical (22, 24). I cannot now review the various advantages which seem to me to result from distinguishing these three frames or reference. I need only say that, just as the hedonistic or adjustive processes are an outgrowth of adaptation as mediated by organic evolution, so is integration, or ethics, an outgrowth of the principles and processes of adjustment and hedonistic learning. In other words, although integrative behavior superficially seems to transcend the principles of adjustment, such behavior has been developed and is socially perpetuated precisely because it is, in the final analysis, *both* adjustive and adaptive.

II

In a similarly synoptic manner, I shall briefly review Rice's paper, singling out for special comment those points which are particularly significant or which seem to me incompletely developed.

1. Writing in 1931, Thorndike (30) referred to the Law of Effect as having been an 'unpopular doctrine' (p. 33). The reasons for its unpopularity are complex and manifold, but prominent among them is the fact that from its inception it was coupled with the Law of Exercise. Between the two of them it is possible to 'explain' everything in the field of learning but to predict nothing. These two 'laws' are completely complementary in that whatever one cannot account for the other necessarily will (24). Convenient as such an *ad hoc* arrangement is, it is not the stuff from which science is made. Any theoretical system which can never be driven into a corner, which cannot be made to stand or fall on the basis of crucial experiments, has a spurious strength which in the end may be its undoing.

It was therefore a great advance when the advocates of the Law of Effect began to discard the false comfort provided by the Law of Exercise and set out to make the Law of Effect stand on its own two feet, or not at all. This development, briefly alluded to by Rice, is constantly gaining experimental support and is of fundamental importance for any discussion of the current implications of the Law of Effect. It is regrettable that Thorndike himself, after having advanced some of the most coercive evidence against the Law of Exercise (30, 31), has shown a tendency in more recent writings to fall back upon his earlier faith in this dubious principle (32).

2. Rice likewise comments upon the revision of the Law of Effect whereby its original bifurcated formulation has been replaced by a single, positive principle. Although it is perhaps too soon to conclude that learning is always and necessarily an irreversible, indelible process, there is considerable evidence that this may be the truth of the matter. After extensive experiments designed to compare the effects of 'satisfiers' and 'annoyers,' Thorndike remarks, "Since in these experiments with these subjects, the wrong connections were simply displaced or nullified by the right ones, not intrinsically weakened, we may properly expect that something similar may happen in many sorts of learning, and we may increase our confidence in positive rather than negative learning and teaching" (30, p. 46).

Important as it is, this finding may, however, be easily misinterpreted, a danger which is not lessened by the equivocal manner in which it has sometimes been presented. From incautious statements of this finding, one can get the impression that punishment has no

effect whatever upon the behavior of living organisms. Such an inference is certainly at variance with the everyday evidence of one's senses. The fact which one needs to keep explicitly in mind in this connection is that, whereas satisfaction appears to have a directly reinforcing effect upon the stimulus-response sequences which precede or accompany satisfaction, the effect which punishment has upon behavior which precedes or accompanies it is a *secondary, indirect* one. That is to say, punishment does not weaken the 'habit,' or 'connection,' which it follows; it merely *inhibits*, or *interferes* with, it through the production of a conflict (11, 21). In short, the punished habit is not 'taken out by the roots' but is simply overlaid, suppressed, superseded by some new and more potent stimulus-response tendency.

When examined in detail, the effect of punishment seems to involve the principle of conditioning, *i.e.*, the kinaesthetic and other impulses which accompany a punished act become danger signals and set up a fear state which can be eliminated only by stopping the act which produces them. The process thus involved is pretty obviously both different from and more complex than the process involved in the straightforward strengthening of a stimulus-response sequence which results in satisfaction.

Although phrased differently, this interpretation is clearly implied in the following passage from Thorndike:

> "Annoyers do not act on learning in general by weakening whatever connection they follow. If they do anything to learning they do it indirectly by informing the learner that such and such a response in such and such a situation brings distress, or by making the learner feel fear of a certain object, or by making him jump back from a certain place, or by some other definite and specific change which they produce in him. Satisfiers *seem* to act more directly and generally and uniformly and subtly . . ." (30, p. 46).

In summary, then, the Law of Effect, as currently formulated involves the single, unitary proposition that living organisms learn when and only when they solve a problem in the sense of reducing a tension, relieving a discomfort, deriving a satisfaction. It assumes that the effect of so-called punishment is simply to supply a second drive which leads to the learning of new habits which inhibit older ones and that conditioning, or associative learning, is a subsidiary and dependent form of behavior modification (20, 26). Such a conception of the learning process has the advantage of articulating easily and constructively with modern clinical theory and anthropological functionalism. It also provides, as will be shown later, a common denominator on which to base a reconciliation of learning theory and Gestalt psychology.

3. Rice is right in singling out the word 'repetition' for special scrutiny both in connection with its implications for the Law of Effect and for Allport's criticisms of that law. It should be reemphasized that, on the basis of the present formulations of the Law of Effect, repetition is important in the production of learning solely in the sense that repetitions—or, more accurately, occurrences—of a given stimulus-response sequence are productive of learning only if they are accompanied by satisfaction. It is not, in short, the repetition of a given act that leads to learning, but rather its *rewarded repetition*. Non-rewarding or punished repetitions of acts lead to their inhibition (21).

Rice suggests that small children, mental defectives, and compulsive neurotics sometimes repeat stereotyped, meaningless, unrewarding acts, but that normal adults do not. An alternative possibility is that the seemingly unre-

warding repetitive activities sometimes seen in the former cases are unrewarding only in the sense that they do not lead to any externally discernible satisfaction but that they serve to *alleviate anxiety,* and thus conform quite acceptably to the hypothesis that behavior is reinforced and perpetuated when and only when it is in some way satisfying (24).

I would criticize Allport's example of the student who gets an 'A' in a college course, and yet does not repeat the course, on grounds somewhat different from those which Rice employs. Note that the Law of Effect assumes that a response which has resulted in satisfaction on one or more occasions will be repeated subsequently only if the problem to which that response provides the solution *recurs.* This law does not for a moment assume that living organisms, after having solved a problem or satiated a drive, go on endlessly repeating —like a cracked phonograph record— the behavior which produced the satiation. As Rice says in another connection, one cannot get "satisfaction from problem-solving unless there is a problem" (p. 311). The student does not repeat a course taken with a satisfactory mark for the reason that the situation or problem which caused him to take the course in the first place has been permanently eliminated. Only under the most unlikely circumstances can one think of the problem's possible recurrence.[3]

4. Rice usefully draws our attention to the fact that the performance of a satisfying action does two important things: (a) it reinforces the connection between the underlying drive and the action which brought the satisfaction about and (b) it leads to the development of what has been variously termed fixation on the goal-object, cathexis, expectation, appetitive conditioning, or interest. Educational writers (34) have stressed the importance of the psychology of interest, and psychoanalysts (7) have similarly emphasized the parallel concept of cathexis; but as far as laboratory experimentation is is concerned, this phenomenon has been generally neglected. It is little wonder that we do not yet have a comprehensive and unified theory of learning!

A variety of incidental observations seems to make it nearly unavoidable that we assume (a) that 'interests' are secondary drives and (b) that as such they can both motivate and reinforce behavior, in much the same fashion that primary drives do (18, 20). It has just been possible to show unequivocally (26) that it is necessary to posit fear as an 'intervening variable' (33) in order to explain certain phenomena of avoidance conditioning; and the necessity seems hardly less great to suppose that appetitive drives, or 'interests,' can function similarly. This means that, even in intra-human organisms, it is necessary to have a theory of learning, and of behavior generally, which explicitly acknowledges the importance not only of the primary but also the secondary drives (19). When this is done, the psychology of learning and ego psychology are brought tangibly nearer to each other; but as Rice suggests, there are no clearly established facts from the latter area which compel even a partial repudiation of the Law of Effect, as the beginning and ultimate guiding principle of all learning. On the contrary, if properly in-

[3] Mr. Emmanual Bellar has suggested (in conversation) a different but not incompatible re-interpretation of Allport's example, namely, that getting an 'A' in a college course may be thought of as comparable to a rat's successfully negotiating a single segment of a maze. Just as the rat is trying to get 'through the maze,' the student is trying to 'get through college.' There would, therefore, be no more reason to expect a repetition of a successful step toward the ultimates goal in the one case than in the other.

terpreted, the facts impressively corroborate such a theory.

5. Once we posit that certain drives, which may function to produce new learning, are themselves the outcome of past learning, we are in a position to account for a number of previously bothersome phenomena. I can refer here to only a few of them.

Rice says:

"The only possibility of saving the Law of Effect, then, would seem to consist in finding *what* is reinforced by success or satisfaction, and therefore *what* it is that tends to be repeated. This implies that we should either replace 'response' by some other concept, such as 'interest,' in the statement of the Law, or else try to find some *aspect* or *feature* of the responses, rather than the response as a whole, which is reinforced by satisfaction; in the latter case, the feature selected must include for human activity, and possibly for some animal behavior, the core of what is intended by the vague term 'interest' " (p. 310).

In the paragraphs which follow this quotation, Rice shows how productively the concept of interest can be used in accounting for very diverse behavior, and he shows equally convincingly that interest itself is a learned phenomenon which follows the Law of Effect. He then concludes:

"The upshot of all this is that the validity of the Law depends on *what* is conceived to be satisfied or thwarted, and therefore reinforced or extinguished. Only because many proponents of the Law, influenced by the cruder forms of reflexology, have focused on response mechanism and goals rather than interests, and have ignored interests of high generality, has their proof of the Law been so inadequate" (p. 314).

The only ambiguity that seems to me to appear in Rice's analysis is his tendency to separate 'responses' and 'interests.' Although the phenomena which he denotates by these two terms are importantly different, yet there is every reason to believe that they are *both responses*, the former being, in the main, overt skeletal responses and the latter being, in the main, covert, emotional responses. This terminology allows us to see 'interests' as learned by past satisfaction in essentially the same way that other responses are learned; it shows how it is that interest-responses, or emotions, may serve to motivate and reinforce new learning; and it identifies the 'goal-object,' actual or symbolized (imagined), as the stimulus, or signal, which trips off, or arouses, 'interest.'

6. One of the most interesting ('emotion-arousing'?) and suggestive passages in Rice's whole paper is the following:

"These remarks fit in with Thorndike's own later statements about the Law of Effect [30, 31], though he has not given a precise reformulation of it. There he has tended to treat reinforcement in terms of 'wants,' 'interests' and 'attitudes,' as well as in terms of stimulus-response connections; and where he does refer to responses he says that reinforcement can attach to any 'part or feature' of them, that the response may consist of an idea, a mood, a liking, a craving, as well as a motor act, and that the reinforcement operates upon the 'relevant' or 'belonging' aspects of the response. And for human motivation, Thorndike stresses the 'confirming reaction' or 'O. K. reaction' as the kind of reward that is most potent. The confirming reaction is strongest when it is a gratification of the ego by way of self-esteem or a gratification of the super-ego by of 'conscience' " (pp. 314–315).

These remarks have prompted me to review the literature on Thorndike's concept of 'belonging,' with the following questions in mind: (a) What were the experimental facts or circumstances which drove Thorndike to posit such a concept and (b) How is this concept related to or different from the Gestalt concept of 'insight'?

The answer to the first of these questions may be very simply illustrated by

reference to a single experiment, which is one among many which Thorndike has reported in this connection (30, 31). Thorndike asked a group of subjects to listen while he read the following sentences ten times:

Alfred Dukes and his sister worked sadly.

Edward Davis and his brother argued rarely.

Francis Bragg and his cousin played hard.

Etc., etc.

At the end of the tenth reading the subjects were asked to write answers to such questions as:

What word came after "Francis"?
What word came after "sadly"?
Etc., etc.

From this experiment Thorndike discovered that his subjects could answer the first type of question much more readily than the second, and from this and related findings he concluded that mere repetition of a sequence of events or acts has little or nothing to do with learning, but that the presence or absence of something which he refers to rather inexplicitly as 'belonging,' or 'belongingness,' is of critical importance.[4]

Reviewing this work, Rock says:

"The principle of 'belongingness' (which Saniford calls 'that horrible word') represents one of the important new concepts which grew out of experimental work on other aspects of learning, and which was itself then subjected to experimental scrutiny. Belongingness is not strictly defined by Thorndike, but numerous illustrations indicate that if the things to be connected 'belong' there is between them some sort of inherent bond, in the sense that the subject feels there is a certain fitness in connecting these things. This principle, which possesses something of both the nature and the vagueness of the Gestalt view, has been investigated or illustrated by some highly ingenious experiments. Certain of the aspects of belongingness are to be understood in terms of mindset, either habitual or temporary, while other aspects are related to meaning, though there appears to be more to the principle than either or both of these" (28, p. 756).

This capitulation on the part of Thorndike, as it seemed to some, was immediately seized upon and capitalized by the more zealous proponents of *Gestalttheorie.* In a pointedly titled article, 'Thorndike's theory of learning as Gestalt psychology,' Brown and Feder announced that "in his latest writings Thorndike, himself long one of the staunchest supporters of psychological atomism, self-styled connectionist, has unwittingly gone over to the other side. Our specific aim in writing this paper is to show that Thorndike's theory of learning could be successfully rewritten in terms of Gestalt psychology without serious modification of the tenets of either" (4, p. 426). Inescapably, 'belongingness' was the central target of this attack, leading to the conclusion: " 'Belonging' or sense of relationship implies organization (i.e., *Struktur,* Gestalt), a property of wholes and only of wholes. . . . Thorndike and the Gestalt Psychologist are talking about the same sort of thing" (4, p. 428–429).

Anticipating reactions of this kind, Thorndike had already remarked in 1931:

"I hope that the sort of connection-system which I have described in these lectures is more acceptable than the kind against which configurationists like Köhler and Koffka and Ogden direct their criticisms—criticisms from which I have profited, and with which I often agree. . . . But I cannot see that such a connection-system requires aid from closure or Prägnanz. The facts which they explain seem explainable nearly or quite as well by

[4] Cf. Hartmann's four lists of words ranging in meaning from nonsense syllables to a complete sentence (10, p. 292).

varied reaction guided by the satisfyingness of the results attained . . ." (30, p. 130–131).

Writing in 1935, we find Koffka sounding, in certain important respects, very much like Thorndike:

"Since all problem solutions can be said to consist in finding the *fitting* part which will relieve the existing stress, a law of fittingness would be the most universal law to explain thinking, and with it the arousal of new processes. Such a law would be a generalization of the laws of good continuation and closure" (13, p. 638).

And in 1943 Thorndike (32), in turn, sounds much like Koffka, although in some respects still very different:

"When one responds to the situation, 'What is the square of 10?' by writing, saying, or thinking '100,' the 100 clearly 'belongs to' the 'What is the square of 10?', is evoked by it, and presumably is physiologically linked to it as a later part of some neural activity of which the earliest part represents the question. This belonging and physiological linkage may be present to some extent, however, when the response has no important fitness or relevance to the situation" (32, p. 25).

The question now is: How can the agreements between Thorndikians and Gestalt students of learning be consolidated and the remaining differences reconciled? One promising possibility will be discussed in the following section.

7. The whole of Rice's third section is devoted to the proposition that "the confirming reaction for humans, and to some small extent for higher animals, can attach directly to symbols" (p. 316), which is to say, if I interpret the facts correctly, that living organisms which are capable of being emotionally aroused by appropriate signs may also be relieved, reassured, rewarded by other signs and that when the latter state of affairs occurs, there is satisfaction, just as there is when a primary drive is reduced or eliminated. Thorndike has called this emotional, or secondary, type of satisfaction the "confirming" or "O. K." reaction. It is, I submit, also essentially what Gestalt writers have in mind when they use the term "insight."

Let us take the familiar case of Köhler's ape, Sultan, contemplating the banana that lies out of reach beyond the bars of his cage. The experimenter has given him two bamboo sticks of about equal length, the ends of which will fit one into the other. After more than an hour of abortive attempts to reach and rake the banana toward him with the sticks, used individually, Sultan abandons the sticks and seems, at least temporarily, to give up his quest for the banana. The experimenter goes off and leaves Sultan under the observation of the keeper but still "in possession of his sticks."

"Keeper's report: 'Sultan first of all squats indifferently on the box, which has been left standing a little back from the railings; then he gets up, picks up the two sticks, sits down again on the box and plays carelessly with them. While doing this, it happens that he finds himself holding one rod in either hand in such a way that they lie in a straight line; he pushes the thinner one a little way into the opening of the thicker, jumps up and is already on the run toward the railings, to which he has up to now half turned his back, and begins to draw a banana towards him with the double stick" (14, p. 127).

Let us abruptly stop this dramatic, rapidly moving picture at this point and review what has happened. The Law of Effect, as we traditionally know it, would certainly lead us to expect that if Sultan thus obtained the banana, on subsequent, similar occasions he would solve the problem in the same way, increasingly promptly and proficiently. But I think we can now predict that if, as Sultan suddenly started fitting the sticks together, the experimenter had

taken the banana away, Sultan would have been scarcely less prompt in seizing and starting to fit the sticks together as soon as he saw the banana, on a later occasion, in its usual place. In other words, there is now no reason to suppose that learning can occur only when a primary satisfaction (such as hunger-reduction) is experienced; current formulations of the Law of Effect include the possibility of learning resulting from secondary, or purely emotional, satisfaction.

Actually, if we read on, we find that adventitious circumstances developed in the experiment with Sultan which afforded a striking confirmation of the foregoing prediction. As it turned out, the smaller of the two sticks did not fit snugly into the larger one, with the result that, before the banana was obtained, the sticks fell apart "several times; each time Sultan rejoins the tubes immediately by holding the bigger one towards himself in the left and the thinner one in his right hand and a little backwards, and then sliding one into the other. [Illustration] *The proceeding seems to please him immensely*" (14, p. 128, italics added).

Eventually Sultan makes the double-stick work and obtains a banana, but there can hardly be any doubt that well in advance of this event the ape experienced considerable emotional satisfaction, which powerfully reinforced the stick-fitting-together response. The only question which remains is how and why this satisfaction came about.

Although we cannot answer this question in detail, we can answer it at least in principle. Whenever a living organism experiences a protracted primary drive, such as hunger, there develops an emotional state which can be characterized as *an anticipation of more hunger*. Thus, to the primary drive is added a secondary drive of apprehension, discouragement, or the like. To take a familiar example, when one is ill one experiences not only the pain and inconvenience of the moment but also a foreboding lest the pain and inconvenience persist or even increase. When the doctor comes, examines us, and pronounces certain symbols, we immediately 'feel a lot better,' even though the physical basis of our condition has probably not altered in the slightest. In like manner, when Sultan was not only hungry but in prospect of continuing to be hungry indefinitely, he, too, 'felt a lot better' when he hit upon a plan, or had an 'insight,' which he believed would bring his hunger to an end, or at least importantly reduce it.

How such 'plans,' 'ideas,' 'insights' develop is still uncertain, but even here we are not entirely in the dark. First of all we know that plans are developed through either sensory or symbolic exploration of the situation and of the possible ways in which the problem presented thereby may be solved. As these possibilities are passed in review (*cf.* the variation of response in trial-and-error behavior), there is a constant appraising, a censoring or approving of them. At the human level, we consider and discard a succession of possibilities, until at length we hit upon one which elicits a "Ha, that's it. Now I have it!" The plan, or 'insight,' may of course, be realistically impractical, wrong; but the self-administered satisfaction, the drop in emotional tension experienced at the moment is none the less real or effective in reinforcing the responses which have preceded it.

Although we know a good deal about the way signs, symbols, and situations may serve to arouse emotional states of various kinds, we know comparatively little about the way in which they may also serve to terminate such states. For example, there have been many laboratory studies of the behavior of living organisms in the presence of signs which

have become 'danger signals,' but there have been few studies of 'safety signals.' However, the preliminary indications (5, 11, 12, 16, 19, 20), are that the latter are no less amenable to experimental analysis than the former, and that such analysis, particularly when carried on from the genetic standpoint, will do much to dispel the mystery implicit in such concepts as the Gestaltists' 'goodness of figure' or Thorndike's 'belonging.'

Despite the unsolved problems which remain in this field, there is already much solid ground beneath our feet. No longer is there any reason for schools of thought to rise and flourish around the question as to whether learning occurs only when a primary drive is satisfied, as early formulations of the Law of Effect seemed to imply, or only when there is "insight," as many Gestalt writers have contended (*cf.* Hartmann's, 9, summary of the Gestalt standpoint). The more tenable and integrative position is that learning occurs when and only when a drive is reduced, a problem solved, a satisfaction derived, but that this satisfaction may stem from the reduction of *either* a primary or a secondary drive. This view is certainly implicit in the following passage by Thorndike:

"Whatever else it may be, thought is a series of varied reactions. As the series occurs, one or another response is selected, emphasized, and allowed to determine the next thought, because it relieves some annoying irritation or lack or satisfies some craving in the thinker. Certain responses are disregarded or discarded as useless or harmful because they fail to satisfy or because they produce actual discomfort. These annoyances and satisfactions are no less real because they lack the sensuous or emotional qualities of electric shock, food, fear, or social approval. To be thwarted in solving a problem in arithmetic is as truly annoying as to be thwarted in getting out of a box to food or companions" (30, pp. 145–146).

As Gates says, ". . . the basal idea of the organism's own potency in influencing its own course of learning is the keystone of the Thorndike [newer] psychology of learning" (8, p. 149).

In similar vein, Krechevsky, an investigator with strongly Gestaltoid leanings, has maintained that behind the so-called random, or chance, trial-and-error behavior which one can observe in animal problem-solving, there is nearly always a secondary, or symbolic, level of activity which results in 'hypotheses' which are then tried out and if objectively unsuccessful are soon discarded for new ones.

Thus, "when we say that an individual has an 'hypothesis' we imply that the individual is contributing something to the situation. His behavior is not something forced upon him by the immediately presented stimuli. He has taken the problem-field and has brought to bear upon it all his past experiences. His 'hypothesis' originates to some degree *from within himself*. . . . An 'hypothesis' is the individual's interpretation of the data, it is not a phenomenon deriving from the presenting data alone" (15, pp. 531–532).

"The question, as Krechevsky points out, is Where does the animal 'get' these 'hypotheses'? The Gestalt theory has one answer. Such behavior, or any behavior is an inevitable result of some stimulus configuration. The situation *forces* a specific response.[5] The relationship between stimulus-field and response is as definite as it is mysterious" (15, p. 531).

Although we do not know the historical facts, we may nevertheless assume, following Krechevsky, that in the case of Sultan, an 'hypothesis' was formed before the animal actually began putting the sticks together and that this hypothesis generated a certain 'faith' that the hunger problem could thus be solved, a 'faith' which was in some way generalized, or transferred, from other problem-solving situations to this one

[5] *Cf.* Rice's apt characterization of the a-historical bias of Gestalt psychology (pp. 318–319).

and which would last for only a limited period of time if it met with consistent failure and frustration, *i.e.*, would, like any other unrewarding reaction, eventually extinguish. Gestalt writers have undoubtedly rendered a real service in calling attention so insistently to learning that occurs without the involvement of *primary* satisfactions, thus forcing proponents of the Law of Effect to expand their concepts to include the possibility of learning through *secondary* drives and rewards; but certain of the Gestaltists' own speculative, or metapsychological, postulates seem increasingly wide of the mark, 'irrelevant,' lacking in 'belonging' and 'fittingness.' Proponents of the Law of Effect have acknowledged their early errors and have made their concessions to *Gestalttheorie.* Are the advocates of the latter prepared to do as much?

8. Because of his preoccupations as a professional philosopher, but equally by the very nature of the facts, Rice cannot fail to point out some of the ethical implications of the Law of Effect. For example, he asks:

> "What can cause an individual to persist in a 'style of life,' or the pursuit of it? Observation suggests that he does so because he has found satisfaction, actual or imaginative, from that style of life, or because it promises to integrate the scattered elements of past satisfactions and lessen past dissatisfactions, or because social rewards and punishments promote its adoption. But all these are operations of the Law of Effect" (p. 313).

Elsewhere (24) I have developed at some length what seem to me to be the salient implications of learning theory for ethics, but it is sufficiently evident, without elaboration here, that as soon as we have a psychology of learning which can speak meaningfully about self-administered satisfactions, and dissatisfactions, we are in a position to begin dealing systematically and scientifically with problems of conduct, conscience, and morality. From the modern psychological clinic and consulting room, as from traditional religion and social precept, a good deal is already known in this area; but there are still many mysteries which urgently call for more precise investigation than has thus far been accorded them.

One may surmise that it was certain of these mysteries which prompted Allport to put forward his concept of 'functional autonomy' (1, 2). It is certainly true that a normally mature man or woman must have the capacity to persist in the face of punishment and to resist in the face of possible satisfactions in a way that implies a kind of immunity to and triumph over the Law of Effect; but as Rice points out, these facts constitute no paradox for the Law of Effect if one sees them in terms of 'interests' of a sufficiently 'high level of generality.'

Scientific students of learning theory are thus coming to acknowledge the importance of 'the moral problem.' Will the professional moralists and theologians be equally amenable to scientific logic and concede that the ultimate *raison d'être* of morality lies in the psychology of learning?

9. This paper makes no claim to being either complete or entirely consistent. There are still many lacunae and paradoxes in our theory of learning. For example, take the problem, instanced by Allport (3), Murray (27), Lashley (17), and many others, of *response equivalence.* Why living organisms do not immediately and permanently fixate upon whatever response or response-sequence has been found to lead to satisfaction, but instead continue to show more or less behavior variability, is still an open question (12). Helen Jones and I (21) have made a number of attacks on this problem, but they have all been unsuccess-

ful to date. This problem, along with many others, is of great systematic significance; but I concur fully in Rice's judgment that, despite the unresolved difficulties which remain, the Law of Effect is by all odds the most inclusive and predictively potent conception of learning which we have, and should be pushed to its limit.

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EFFECT: A SECONDARY PRINCIPLE OF LEARNING

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So deftly and so incisively have Dr. Rice and Dr. Mowrer handled certain thorny problems of the law of effect that I hesitate to add my own comments lest in my efforts to clarify I may unintentionally becloud the issues that they have treated so ably.

The subject of this symposium, as all readers know, is baffling in its complexity and in its ramifications. The law of effect can scarcely be treated apart from the protean problem of learning as a whole, and this vast problem soon leads one into philosophical presuppositions which are as unavoidable as they are distasteful to most psychologists. Experiments in this area, though legion, are unfortunately not compelling so far as their theoretical interpretation is concerned.

In order to avoid as well as I can the pitfalls in the way, and to keep the discussion within the bounds set by the two preceding authors, I shall confine myself strictly to comment on the observations and interpretations offered by each of them. The reader will recognize that the discussion in this symposium is at a fairly high level of abstractness; only the broader issues are touched upon. Detailed reference to single experiments must, for the most part, be avoided lest our perspective on the problem as a whole be lost.

Dr. Rice's Position

Although the following summary of Dr. Rice's views is sketchy, it is, I trust, sufficiently accurate to remind us of the principal features of his trenchant argument (8).

In the past, says Dr. Rice, the law of effect has been inadequately formulated, yet in a revised form it may retain the central position accorded it in learning theory during recent years. If not actually the 'sole principle' of learning, it, along with the law of exercise, and a few other minor principles, may be considered virtually sufficient.

Its primary weakness has been the assertion, or at least the implication, that a specific response becomes set through the operation of a satisfying outcome. Such a picture is rigid and false. While something of this sort may seem to exist in animal behavior, it obviously does not apply to human reactions complicated by systems of evolved interests. For example, men who are self-assertive will seldom repeat successful acts but will seek new means to satisfy their mounting level of aspiration. Men who like novelty will deliberately shun a repetition of acts; so too will men who are set to solve new problems (since stereotyping of acts cannot contribute to new solutions).

In short, "the law cannot be upheld if it means that success or satisfaction necessarily enhances the tendency for either or both (a) the specific response sequence to be repeated, and/or (b) the particular, or even the specific, goal object to be chosen again." He agrees with my previous criticism that rigidly repetitive conduct is characteristic only of very young children, mental defectives, and compulsive neurotics (1). What adults normally do is to vary both goal and responses while holding only to the same general *kind* of response and selecting the same *kind* of goal that has brought good results in the past. The law seems to apply better to *similar* systems than to *identical* sys-

tems, at least in normal adults, and especially in those who are self-assertive, interested in novelty, or trying to solve a new problem.

Problem-solving is important. The reason men do not repeat endlessly their past success is that having succeeded, the specific problem and motive vanish. A student will not repeat a course in which he obtained an A, because he no longer has this problem to solve. When a new problem arises new determinants inevitably enter. All that success or satisfaction does is to confirm one's interest in the general range of problems (mathematical, amatory, professional).

Self-assertion, likewise, is important, for as a motive it keeps the person in a chosen channel of conduct (persistence) in spite of repeated failures. Though not succeeding (one meaning of 'effect') the person derives satisfaction (another meaning of 'effect') from his trying. For some people "to fail at a difficult task gives more satisfaction to the ego than to succeed at an easy one."

Granted that self-interest (ego-involvement) seriously alters the application of the law of effect as traditionally stated, it is none the less true that the law has a demonstrable influence upon the development of the ego itself. "Though the depth psychologists have given little explicit mention to the law of effect in dealing with such matters, their stress on the importance of satisfaction and frustrations, rewards and punishment, in character formation, suggests that the development of the ego itself may be subject to Thorndike's law." Punishment to the ego sometimes deadens it—as the ultimate brutalities of concentration camps occasionally succeeded in doing. Some form of reward is necessary to confirm an individual in his style of life and in his system of interests.

Novelty may bring satisfaction, and if it does so, it too will violate the rigid law of repetition-of-rewarded-response. Only non-recurring responses give satisfaction.

All in all, the validity of the law must depend "upon what is conceived to be satisfied or thwarted, and therefore reinforced or extinguished." Normally neither specific goals nor specific means are reinforced, but rather systems of interests become confirmed through experience of satisfaction.

New interests evolve from old (as, perhaps, an interest in poetry from an interest in sex), but this evolution of functionally autonomous motives is itself dependent upon the satisfactions that the *new* line of activity yields to the person.

Since it is often difficult to find rewards in the environment which would explain man's persistence in the face of apparent failure, or supply an evident reason why a given system of interests is so gratifying to the individual, we must invoke, with Thorndike, the concept of self-administered rewards—the O.K.—delivered by the person to his own conduct.

Whether such reinforcement is applied to an interest system and course of conduct as a whole, or whether there is some specific feature in the chain that is controlled by the law of effect is not known. Rice proposes the possibility that the 'core of the act' is symbolized for the individual in some way, and that the O.K. is directed to this core. In this manner the *purpose* behind an act can be rewarded (by the self or by others), rather than any overt sequence of responses. Rewarding the response is really less important than rewarding the purpose: "We show great flexibility in adapting our means to our ends, and we may change our goal-objects freely provided that the pursuit of them is expected to provide, somewhere in the

process, those qualities or structures of the act to which satisfaction has adhered."

Through symbolic responses we may weave our approval back and forth between purpose and anticipation of response, and thus "as we are carrying out the successive phases of the act, we keep the symbolized purpose in mind, mull it over, and confirm it repeatedly; unswerving concentration on this purpose, as it begets auxiliary goals, is necessary to keep us going."

Confirming occurs continually along the way, so that it derives benefit from the 'gradient of reinforcement.' External reward is often slow to arrive or never occurs at all, but inner self-administered reward is immediate and therefore effective. Purpose and interests receive on the whole more reinforcement than do actual motor skills and conduct patterns. Hence we should expect the law of effect to apply to interests and purposes more than to specific responses. In sub-rational learning, however, the gradient effect attaches to the reward of ultimate and penultimate responses (as experiments with animals show).

Rice admits that motivation must always be conceived as operating in the present, and that dissatisfaction rather than satisfaction is the crux of motivation. "The dynamic character of an act in progress . . . derives from an immediate dissatisfaction, ... But the interest as an enduring disposition . . . will be reinforced by repeated satisfaction. The law of effect, then, is a law of retention of interests or dispositions; it does not deal with the mechanism of the particular striving, except in so far as that is determined by past fixation or interest as one of its conditions."

Finally the operation of insight, understanding, belongingness, in learning may be explained provisionally in terms of the success that attends good solutions via the self-reward of symbols: "reinforcement may attach to the 'relevant' or 'required' features of the act."

Agreements with Dr. Rice. If my summary has been fair, I record my basic agreement with the position taken. I believe Dr. Rice is thinking in the right direction.

One feature deserves special commendation. Dr. Rice attempts, more pointedly than most authors, to account for the *refashioning* of motives. He believes the liberalized law of effect is indispensable for this purpose. Instead of assuming a rigidity of drives or instincts throughout life, he sees clearly that almost endless derivative motives must be themselves explained as products of learning. Though adult psychogenic motives are derivative they are in no sense *functionally* secondary. He holds therefore that new motives set up in their own right in proportion as satisfactions of some sort sustain them. Learning thus accounts for motives, not merely for skills and means.

Dr. Rice further spots clearly the defect in the concept of 'success.' It is certainly not objective success alone that helps to sustain a motive. The only reward that is operative in many cases seems to be a purely subjective sense of satisfaction. A person who persists in a hopeless task by sheer grim determination, or perhaps from a sense of duty, probably derives some satisfaction from doing so, but no success. We note, however, that in such a case 'satisfaction' is a rather vague word, for along with subjective satisfaction we find plenty of painful, frustrating, punishment, whose effects somehow or other are assumed to be offset by an inner O.K.

I agree with Dr. Rice that the traditional statement of the law of effect seems to apply much better to children, imbeciles, and animals than to normal human adults. To the latter it ap-

plies badly, excepting in routine, 'blind' learning which is wholly peripheral to the central interests of a life.

I also agree that satisfaction plays *some* part in the development of central interests and purposes. An individual does mull over his purposes and plans and unless he approves of them (*i.e.*, rewards himself symbolically) he usually, in the long run, discards them.

If the law of effect can be held in this somewhat amorphous and plastic form, I favor it. But Dr. Rice, I feel, does not see fully how greatly he has attenuated the law, and how much he is forced to leave to other principles of learning. Dr. Mowrer, I believe, does see the consequences of this attenuation, and draws back into a far more conservative position, lest through conceding too much he discover that he has relegated the law to a secondary position in the economy of learning.

Problems unsolved by Dr. Rice. I should like next to call attention to certain aspects of learning (particularly the learning of motives) which elude the law of effect as Dr. Rice employs it.

He asks what can cause an individual to persist in a 'style of life.' And he answers, "Observation suggests that he does so because he has found satisfaction, actual or imaginative, from it." This statement is acceptable enough; but when we ask *why* does he derive satisfaction from it, we are again at sea. There must be formative influences (hereditary perhaps) that dispose some of us to find satisfactions from aesthetic, athletic, or humanitarian styles of life. The problem may seem to lie beyond the scope of the present discussion, yet it is really relevant. Manifestly we learn best that which fits our own style of life, but this style turns out to be determined by antecedent conditions more basic than satisfying experience. Hence effect cannot possibly be the *only* law of learning. At best it operates upon systems in part established for other reasons.

Dr. Rice admits the satisfaction that comes from varying one's performance, from non-repeated means-end solutions. He proposes that novelty itself is rewarded, but the old law of effect claims only that a "response is rewarded." How can it also claim that non-repetition of a response is rewarded? To do so would be to surrender completely the conception of a 'stamping in' process or of a 'retroflex arc.' Dr. Rice is clearly not following the classic conception of effect.

To account for learning by insight, Dr. Rice proposes that reinforcement attaches to the symbolized 'relevant' or 'required' features of an act. I am unable to follow his argument here, because insightful solutions must have occurred *before* they can yield reinforcement or satisfaction. I shall return to this problem, but mention it here to raise once more, and in a different way, the question whether satisfaction can be as basic a condition of learning as even Dr. Rice's moderate position would have us believe.

DR. MOWRER'S POSITION

First Dr. Mowrer wishes to re-establish the argument on familiar ground (7). He therefore takes pains to eliminate concepts which imply that new principles of learning may be needed. For him the *ego* is a matter of *interests*, interests a matter of *emotional arousal*, and emotional arousal a matter of *ordinary drives* whose tension when reduced gives rise to the operation of the law of effect. A bit hurriedly he makes this series of reductions so that 'ego-involved behavior' becomes redacted into the 'familiar drive mechanisms.' Back on familiar ground Dr. Mowrer then starts his argument proper. He believes that the law of effect provides us with "the firmest foundation on which to develop

a truly adequate and comprehensive psychological theory."

His thesis is that "living organisms learn when, and only when, they solve a problem in the sense of reducing a tension, relieving a discomfort, deriving a satisfaction." This is the modern statement of the law of effect which Mowrer tenaciously defends.

Though his defense is tenacious, he realizes that many difficulties arise in the application of the law. For example, it does not tell what happens when both kinds of effects follow an act—when consequences are both rewarding and punishing, pleasurable and painful; it does not tell what happens when consequences vary in time, the effects being both immediate and remote. In such complex cases the effect must be mediated through symbolic processes of some type. The symbolic processes when well developed may be said to constitute the 'ego' and they often result in a *seeming* defiance of hedonism and the law of effect. Especially when integrative or ethical conduct is in question we find much apparent repudiation of pleasure-seeking. Yet ultimately the integrative or ethical aspects of ego-behavior are 'an outgrowth of the principles and processes of adjustment and hedonistic learning.'

There seem, then, to be no real exceptions to the proposition that "satisfaction is the cement that makes learning stick." And by a satisfaction Mowrer means pleasure and drive-reduction. This unrelenting allegiance to a hedonistic version of the law of effect reminds one of the original Thorndikean statement that "pleasure stamps in." Dr. Mowrer is orthodox; even more orthodox than Thorndike, for he has little use for the law of exercise, and even less for 'belongingness.'

One way to bring the psychology of learning and ego psychology together, Dr. Mowrer believes, is to admit "that 'interests' are secondary drives and that as such they can both motivate and reinforce behavior, in much the same fashion that primary drives do." Here Dr. Mowrer accepts Rice's demonstration that interests are a learned phenomenon (learned according to the law of effect) and they function to produce new learning.

In some connections Mowrer speaks of interests as 'cathexis,' in some as 'secondary drives,' but he much prefers to think of them as 'covert, emotional responses.' "This terminology allows us to see 'interests' as learned by past satisfaction in essentially the same way that other responses are learned." It seems to turn out, therefore, that interest-systems are nothing more than emotional responses, and thus introduce no new problems in learning.

Like Dr. Rice he endorses Thorndike's theory of self-administered rewards. In fact, it would not be possible to explain how a course of conduct, expecially of the ethical order, can be sustained, unless frequent self-administered accolades are involved.

Also like Dr. Rice he believes that the Gestalt principle of insight is to be explained by our OK'ing signs and symbols which are related to the reduction of tension or anticipated reduction of tension. The tension relieved may be secondary, and not necessarily dependent on a primary drive such as hunger. There is a constant appraising of hypotheses, censoring them and approving them along the way. Such manipulation of 'danger signals' and 'safety signals' helps us "dispel the mystery implicit in such concepts as the Gestaltists' 'goodness of figure' or Thorndike's 'belonging.' "

Satisfaction is derived in various ways; from reducing tension of a primary drive, from solving a problem, or from reducing a secondary drive. We must also admit secondary drives of a

'high level of generality' to account for the capacity of mature men and women to persist in the face of punishment. But they are not immune to the law of effect if we assume, as Rice does, that self-administered awards sustain these systems of interest.

Unsolved, to Dr. Mowrer's mind, is the riddle of response equivalence, "why living organisms do not immediately and permanently fixate upon whatever response or response-sequence has been found to lead to satisfaction, but instead continue to show a more or less behavior variability." The reason response-equivalence poses such a difficult problem seems to be that Dr. Mowrer does not really regard interests of a 'high level of generality' as true systems. If he did so equivalence would offer little difficulty (since the range of equivalence would define the degree of generality). He is clearly thinking of interests, at bottom, as a 'covert emotional response.' So long as he does not fully accept the existence of generalized autonomous interest-systems the riddle of equivalence inevitably troubles him.

Agreements with Dr. Mowrer. With Dr. Mowrer's purpose and intent I am in agreement. He wishes to find a sound, comprehensive, and adequate theory of learning. He is willing to consider open-mindedly all evidence, no matter how great a strain it may place on the law of effect. He does not insist upon holding blindly to the paradigm of animal learning even though he personally finds it rewarding and genuinely analogical with human learning. A monumental series of experiments conducted by him underlies his theories. He admits the attenuation that must come in learning theory when one considers the conflictful, symbolic, delayed, and ethical character of human conduct. Valiantly he strives to include these complex forms of learning into his theory, and to make room for phenomena that have been hitherto neglected.

Up to a point I agree with him also on the importance of effect. In animals, young children, and in the relatively mechanical and blind learning of human adults the course of learning seems to follow fairly well the traditional statement of the law. And even in interested and ego-involved behavior I would not deny that satisfaction has *some* relation to acquisition of skills, knowledge, and new motives.

I agree also that ego-processes, whatever their nature, are not 'ultimate, unanalyzable, lawless' (Dr. Rice's terms). As Mowrer says, they develop gradually as one ascends the phylogenetic and ontogenetic scales.

EGO-PROCESSES ARE UNAVOIDABLE IN LEARNING THEORY

Dr. Mowrer's rapid reduction of ego-involved behavior to 'familiar drive mechanisms' seems to me invalid. His first step is acceptable enough, namely his insistence that the ego is not substantive, but merely a matter of process. But it is no *more* necessary to use 'ego' adjectivally than to use 'drive,' 'personality,' 'reward,' or 'intelligence' adjectivally. The ego is as valid and as necessary an intervening variable as an other. Experimental evidence shows this to be the case (1).

The second step in his reduction is much too hasty. Though ego-processes can, I agree, be considered equivalent to interests (Dr. Rice's definition in terms of a *system of interests* being acceptable), one cannot safely go further in the reduction. There is, I submit, something functionally irreducible about interest-systems, even though they are continually changing. Nor is it true, in my opinion, that interest is, as Dr. Mowrer says, a familiar term in systematic psychological literature. On

the contrary, one of our greatest defects is our lack of consistent or adequate theory of interest. That interest is not the same as 'cathexis' I stoutly maintain, for interest is a motivational term (interest residing in the organism), whereas cathexis is the superficial doctrine that this or that object becomes attached to some permanent (and usually infantile) drive or instinct. The cathexis theory denies the authenticity of acquired psychogenic motives (that is to say, their functional autonomy).

Least satisfactory of all is Mowrer's reduction of ego-involvement to emotional arousal. Only *certain* emotional states are ego-involved. Literature on concentration camps proves the point over and over again. Frightful pain, terror of one's life, extreme hunger, may be perceived as 'not happening to me'; while a slap, a verbal insult, a trifling humiliation may cut to the quick, and affect the entire ego-structure of the individual. How often in recent years have wartime psychiatrists told us that strong egos handle the most intense emotional arousals, whereas weak egos are undone by trifling emotional arousals? Something here is playing a part besides mere emotional intensity.

Or consider the course of everyday life. When interest is high I find that I am learning smoothly and rapidly. At such times I certainly do not feel emotionally aroused, nor are the familiar drive mechanisms sensibly involved. Indeed when these drive mechanisms with their attendant emotions operate, then my learning is actually *interrupted*,—when, for instance, the needs for food, elimination, fresh air, or riddance from an annoying insect dominate my behavior. Drives are normally peripheral to my ego-interests, and however important they may have been for my infant learning, they seem now to impede rather than advance my adult learning.

I do not deny that some emotional arousals are ego-involved (anger, for instance, usually is). But the two states are by no means identical. Therefore, any law of learning based on 'reduction of emotional tension' does not necessarily apply without modification to learning that proceeds from ego-interests. To this subject I shall shortly return.

Neither Dr. Rice nor Dr. Mowrer denies the existence of ego-processes in some sense, though both wish to show that these processes are regulated by the law of effect. The picture they give for complex learning is like Thorndike's. *We* find *ourselves* confirming *our* purposes, symbolized in some way to *ourselves*, by administering to *ourselves* an O.K. In view of this welter of ego references it would indeed be ungracious to deny the ego some place in learning theory. We will, however, for present purposes pass over the epistemological dilemma into which even 'objective' psychologists inevitably fall, and limit ourselves to the role of the ego in Rice's sense of a 'system of interests.'

Take the case of interests that lead to grim persistence in spite of failure. No single response is rewarded, unless Dr. Mowrer is right in holding that the very core of self-esteem is itself some kind of symbolic response. But whether we regard the ego as a matter of specific symbolic cores (Mowrer) or as a system of interests (Rice), or as the sentiment of self-regard (McDougall), the fact remains that *it* (however defined) must be satisfied in order to maintain a course of conduct. No other responses need to be rewarded for learning to occur, excepting only the ego-response. This fact once more demonstrates that there is a special and selective part played in learning by ego-processes. No learning theory can do without them.

Again, as Dr. Rice points out, after

success or satisfaction we tend to adopt *similar* goals or *similar* acts in the future, but usually not both. If we retain our goal we vary the act (toward greater efficiency); if we retain the act it is usually in the service of an enhanced goal (as in the level of aspiration experiments). How can we explain this characteristic upward push in efficiency and goals unless we assume that the ego is playing some part over and beyond the repetition of a rewarded response? Dr. Rice correctly says that repetition of successful acts is ordinarily a mark of infantile, imbecilic or pathological behavior, rather than of intelligent adult activity. Blind repetition of rewarded response occurs only when the ego-structure is undeveloped or damaged.

THE QUESTION-BEGGING CHARACTER OF 'SATISFACTION'

Both authors concede that in complex learning satisfaction often means nothing more than self-approval. We O.K. our own behavior. Or we find that a system of new interests is satisfying us. We are pleased with what we do and continue to do it (or something similar) because we are pleased. Does such reasoning carry us very far? We beg the question when we say that we do what we do because we are satisfied in doing it.

Dr. Mowrer hopes to avoid this circularity by holding fast to drive-reduction. Hence he defines satisfaction as 'the subjective consequence of solving a problem.' But why then, we ask, does one persist with unsolved problems? He would, I suppose, reply that we reward ourselves at each step for persisting. (If so, and if the law of effect were literally true, ought we not take the first step over and over again?) But the real difficulty here is that there is no independent evidence that we do in fact reward ourselves. Introspectively considered the use of self-applause is so rare and so capricious that it cannot possibly sustain the heavy load that Mowrer, Rice and Thorndike are putting on it. In any case the danger of circularity is still present: we infer from our persistence that we are rewarded, simply because we persist. No independent criterion of reward or satisfaction exists.

The reef is the same one upon which all hedonism is wrecked. Man works (and learns), it is said, because of the pleasure attained. When we ask why a martyr goes to the stake, why a bomber makes suicide dives, why an anchorite forswears all earthly joys, why a member of the underground keeps silence in the face of torture, we are told that they are seeking pleasure —paradoxical though it seems. Insofar as Dr. Mowrer equates satisfaction with pleasure, he too falls into the same trap of *claiming*, without proof, that whatever a man does is *ipso facto* pleasure-seeking.

Nor does 'drive reduction' solve the problem. It is too easy to demonstrate that learning takes place when no drives have been reduced. Suppose while using a cleaning fluid I am careless with a match and an explosion follows, destroying my house and possessions. I shall certainly learn my lesson, but what drive has been reduced? Suppose I mispronounce a word in a public speech with the result that I am ridiculed, and suffer mounting shame and discomfort. Tension has been *created*, not reduced; *dissatisfaction* and not satisfaction has resulted; but in this sequence of events I shall surely learn the right pronunciation. True, I hope to avoid such suffering in the future, but there is *as yet* no drive reduction. In the year 1940 I read and learned the essential contents of *Mein Kampf* with increasing emotional tension, mounting

discomfort, and acute dissatisfaction. Where was the law of effect?

These, and countless similar examples, contradict Dr. Mowrer's statement that "living organisms learn when and only when they solve a problem in the sense of reducing a tension, relieving a discomfort, deriving a satisfaction." If the examples I have just given can be manipulated to fit the hedonistic formula, then I maintain that the formula is so loose as to be worthless. In no intelligible sense in any of these cases was a tension reduced, a discomfort relieved, or a satisfaction derived.

CLASSICAL OBJECTIONS

Neither author alludes directly to the two standard objections to the law of effect: (1) that satisfaction, being both subjective and psychical, cannot legitimately intervene as a causal factor in refashioning neural states; and (2) that effect is a complete anachronism, since the attainment of satisfaction or pleasure *follows* after the crucial series of activities, and therefore cannot well work *backward* in time (especially when a long interval of time intervenes between act and effect).

It may be that the first of these venerable objections is met by an unexpressed assumption that satisfaction has some unknown physical basis which does the actual work in modifying the equally unknown physical basis of learning, so that interactionism need not be assumed. The second objection, Rice and Mowrer seem to answer through a somewhat tenuous reference to symbols. (What they would say in the case of non-symbolic learning I do not know.) Movements leading toward eventual success are accompanied by anticipations, which, being pleasant, invigorate, sustain, and reinforce the movements under way. Some such anticipatory process may well occur, but if so, it presupposes a vast amount of foresight, intention, purpose, imagination and imagery. These presuppositions are so numerous that effect certainly cannot be said to be the sole or primary condition of learning. In order to operate it would require the ability to see relations, an intention to learn, foresight, and a sense of what signals are relevant and which irrelevant to the anticipated path to an imaged goal. There is a deal to learning besides the law of effect!

WHAT SATISFACTION DOES

Up to this point my comments have been chiefly negative. Yet I am as eager as Dr. Mowrer and Dr. Rice to discover and accord to satisfaction its rightful place in learning theory.

The evidence, as I read it, establishes two facts.

(1) It is easy to see that in animals, small children, mental defectives, and in some peripheral phases of adult human conduct, rewarded responses tend to be repeated. The bulk of experimental work on the law of effect has been done with animals, and for this reason the part played by reward in the fixation of response looms large (too large) on our theoretical horizon. Close examination of the experimental results reveals that even in these simplest instances of non-symbolic learning the repeated response varies within a narrow range. Even at its best, then, the law of effect works by approximation only.

(2) In less mechanical forms of learning, satisfaction recedes in importance. When invoked it tends to be a question-begging concept. In any case its operation is secondary to the operation of a variety of other conditions of learning.

What seems to happen in normal human learning, beyond the infant stage, is that experiences of satisfaction serve as *indicators*, which, valuable as they are to the individual, are not dynamically decisive. If I am trying to be-

come a writer and am downcast by a rejection slip, I *may* thereupon cease the style of work that I was attempting. Or I *may* search the slip for encouragement between the lines, and thinking I have found it persist in my style, varying it for a better effect in the future. Knowledge of results is useful in telling me whether I am getting toward my goal. Good news tells me I am on the right road. Yet I *may* be so sure of myself (in my ego-structure) that I will persist in the face of bad news. To say that I am really 'satisfied' by bad news, or that I offset it by giving good news to myself, is dangerous reasoning. The rejection may be bitter to bear; my self-administered praise may be pallid and faint. Would it not be truer to say that in persisting I am refusing to use the indicators of pain and pleasure, and am treating 'these two impostors just the same'?

Satisfaction and dissatisfaction then are useful indicators, but according to the nature of my ego-structure I treat them in a variable manner. Having had a good meal at Restaurant X, I am likely to return when my hunger drive is uppermost. But having been highly satisfied with a score of 89 at golf, I risk failure by setting a goal of 85 for myself. Or, in carrying out some plan in which I meet a painful rebuff, I decide, conditions being what they are, that I cannot afford to take this cue but must persist in my conduct and risk repeated rebuffs. In all these cases there are other determinants at work beside the useful but not decisive indicator of effect.

I think Carr was essentially right in holding that the consequences of an act help us in the future to *perceive* the situation giving rise to the act in an altered way (5). Effect thus becomes *one* of the factors in the perceptual situation. This view, incidentally, helps us escape the paradox of the retroflex. Satisfaction does not stamp in after the fact; it merely alters the determining situation when next it (or a similar situation) recurs.

Although Dr. Mowrer views all learning as a matter of S–R connections, he seems consistently to discount S and to magnify R. To his way of thinking an S is useful chiefly to 'trip' off a response. To my way of thinking the S (including external and internal pressures) is far more decisive.

EFFECT: A SECONDARY CONDITION OF LEARNING

In view of these many difficulties, I submit that the term 'law' is too flattering a designation to apply to the variable operation of consequences on behavior. A law should have greater and more uniform subsumptive power. The 'law of effect' is, of course, an entrenched habit of professional speech, but it would be far more accurate at the present time to speak of a *condition* rather than *law* of learning.

From the foregoing argument it appears that effect cannot be considered the primary condition of learning, certainly not its sole condition. As applied to complex forms of learning its weakness lies in its two ruinous ambiguities: *satisfaction*, as we have seen, is often a question-begging concept; and *symbols* (hopefully invoked to represent some hypothetical core-response which is somehow self-rewarded) are vague molecular constructs that taper off into a kind of aimless triviality so far as explanatory power is concerned.

To hold, as I do, that effect (as an indicator in the perceptual situation) constitutes a secondary condition of learning is, of course, to invite the natural question "What then are the primary conditions?" Since this query opens up the entire problem of learning with its crowded history of attempted

solutions, it falls beyond the scope of the present discussion.

Yet we may hazard the opinion that an adequate learning theory will have to allow a prominent place to the following basic phenomena.

Typically a person learns when he is trying to relate himself to his environment, under the combined influence of his motives, the present requirements of the situation, active participation, and a knowledge of relevant facts, including a memory of his previous success and failure. In this process the role of any previous specific response (rewarded or unrewarded) is not decisive, but is only one determinant among many.

Occasionally, but not often, the process of relating oneself to the environment is so simple that only one segmental drive requires a reduction of tension (for example dust on the cornea leads ultimately to successful blinking or use of the handkerchief, and these responses under like circumstances tend to recur). In such conditions of low complexity, and in animal learning, where ego-structure is undeveloped or, for the occasion, is not engaged in the activity in question, the traditional statement of the law of effect applies fairly well.

One primary condition of learning, not reducible to effect, is the influence of motor activity and participation. Even simple motor involvement speeds up learning (6), but ego-involved participation speeds it up still more (2). There are other familiar conditions of learning which still resist various attempts to reduce them to effect, *viz.*, recency, primacy, exercise, temporal contiguity (classical conditioning), and what Tolman calls 'sign-significance.' A certain amount of 'incidental learning' is likewise unaccounted for.

Further, before effect can be invoked as a principle of complex human learning, several *preconditions* must be assumed. In addition to the motive power of psychogenic interests, one must assume that organisms have a power of administering rewards through a self-conscious and reflective act; that they are able to imagine, foresee, and anticipate goals before they are reached; that they have 'faith' in imagined solutions; that they can test for relevance and fitness of a proposed act.

The assumption of so much rational equipment leads again into the problem of dynamics. It has never been successfully proved that complex learning may proceed without what Leibnitz called an 'active intellect.' In modern times this condition of learning has been represented under such terms as *Gestaltdrang, closure, curiosity, structuring, figure-ground, trace organization,* and *pursuit of meaning.* One thinks here too of Tolman's 'law of emphasis.' It seems impossible to write a comprehensive account of human learning without invoking intellectual dynamisms of this order. To my way of thinking, the interpretations of the law of effect offered by Rice and Mowrer *presuppose* them.

Above all, learning seems to follow the channels of acquired interests. Complex interests bear little or no resemblance to the 'familiar drive mechanisms,' even though they may originally have derived therefrom (3). It is indefensible to speak of them as 'secondary drives' unless the term secondary is used in a strictly chronological sense. Mr. X, let us say, has, in addition to his quota of primary segmental drives, a series of interests in his children's welfare, in fishing trips, in philately, in the cleaning and dyeing industry, and in Catholicism. He learns almost everything that crosses his path provided it has any perceived relevance to any of these interests.

Interest, in this sense, seems to op-

erate almost like a sponge. Anything that has interest-relevance (= ego-relevance) is absorbed—subject, of course, to the limitations of fatigue, intellectual capacity, clear perception, and other similar conditions. What is learned on the basis of this interest-relevance sometimes serves to reduce tension, but often has some other effect. Occasionally, for instance, it serves to increase tension, as when a pianist is memorizing a program for his concert début. But always the learning has some important relation to an interest. The only statement we seem able to make at present is that an interest causes learning which somewhere fits into the interest-structure.

It is true that, in the long run, continued punishment and dissatisfaction are likely to weaken or eliminate an interest system, just as the law of effect maintains. We have evidence, however, that often this result is brought about only at the price of destroying the entire ego-structure of the individual. In concentration camps it often took three to five years of uninterrupted punishment and pain to break down the desire of a person to 'remain the same' (4). In the short run, learning proceeds because it is relevant to an interest-system: it adds to knowledge, it differentiates items within the system, it broadens the range of equivalent stimuli, it does all sorts of things that tend to complete, to round out, or render the interest incisive. Pleasure attending a single response, or even concatenations of response, is not decisive.

Interests, of course, are not separate and unrelated systems. They interlock and comprise the structure of a personal life. The best designation for the resulting *pattern* of interests seems to be 'ego-structure.' Here is our reason for saying that ego-structure is a far more fundamental condition of learning than is the law of effect.

Yet, I repeat, satisfaction often enters into the process of learning as an indicator to the individual that his behavior is or is not appropriate to his own ego-structure. Dissatisfaction characteristically attends his failure to relate himself to his environment adequately; satisfaction accompanies some successful move or anticipated move. Yet it is common for the ego-structure to be so organized that these indicators are disregarded. Interests often persist in the face of continuous dissatisfaction and failure. If liberties are taken with the meaning of 'satisfaction' so that anything a person is doing is said to yield him satisfaction, then the term becomes so broad that it is question-begging, and loses its explanatory value.

In fine, effect is a useful indicator, sometimes playing a role in the total perceptual situation that guides the individual in the pursuit of his interests. But its role is contingent upon, and in this sense secondary to, the total psychological and environmental situation that prevails.

SUMMARY

The three authors in this symposium are unanimous in their desire to establish a learning theory fully adequate to the phenomena of complex human adult learning. They agree in seeing certain defects in the traditional formulations of the law of effect. They all mark, for instance, the fact that rigid repetition of successful responses practically never occurs, and that the law seems to work principally by a curious approximation, affecting 'similar' responses, 'equivalent responses' or a whole 'system' of behavior.

Dr. Rice believes that interests are the key to learning, but that they themselves are the product of past satisfaction or dissatisfaction. Dr. Mowrer believes that interests are re-

ducible to the familiar drive mechanisms and attendant symbolic core-responses. For the former author effect is a primary law of learning, for the latter author, the sole law.

My own argument holds that the vagueness that must result from extending the principle of satisfaction to cover all phenomena of learning (at the higher level of complexity) is such as to disqualify it as a 'law.' At certain low levels of mechanical learning, it may suffice; but at the adult human level satisfaction is at best a *cue*, of quite secondary importance, and often disregarded. Its effectiveness depends on other more important conditions of learning. Of these the interests that comprise the ego-structure of the individual are clearly dominant.

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TECHNIQUE FOR ANALYSIS OF A HIGHLY GENERALIZED RESPONSE PATTERN

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Sooner or later any systematic treatment of behavior runs up against the ability of living organisms to respond to combinations or patterns of stimuli. One can approach the problem from above, in a *geisteswissenshaftliche* direction, or from below, as is more characteristic of American psychological theories, especially the mathematical theories. If one should choose the latter, he can begin very far below, with the response of the single neurone, as in mathematical biophysics (1, 7, 8), or at the level of simple but nevertheless molar activity of the whole animal, as Hull (2, 3) and others do. The test of any theory is not, of course, how shrewdly behavior is 'reduced' to the chosen elements of analysis, but rather how completely the behavior is accounted for when the elements are put together again. Whether one begins at the neurological level or the simple S–R level, before the theory reaches the level of complexity represented by a man buying a pack of cigarettes, the equations must become extraordinarily complicated, if we may judge from published results (which are still only programmatic and in which the most convenient assumptions are made), unless the details of the theory converge upon some relatively simple general principles by which the problem area can be entered at a level not too far below the level of the behavior to be analyzed.

Perhaps the best attack on this troublesome but persistent problem of patterned behavior is to commence boldly at a very high level. The number system, for instance, is usually considered a highly generalized, abstract pattern and, as a consequence, the responses of educated human beings to a series of numbers would illustrate patterned behavior at as highly generalized a level as one could wish. Such responses, as they are made by different people, are end-products, to be sure, of many different kinds and amounts of experience, motivation, abilities, and the like. Yet the very abstractness of the number system imposes an order upon these responses which permits, even facilitates, theoretical treatment. It is possible to analyze the responses of college students to numbers in a quantitative way on the basis of established principles of learning, more particularly of generalization, without becoming mired by a multitude of details.

The first case to be considered is the one in which the subject is presented with a few numbers, say 21, 16 and 34, with instructions to select one of them. Which one will he select? To eliminate idiosyncrasies of number choice we can set this task to a large sample of subjects and, if we avoid numbers like 7 and 13, we can confidently predict that each number will receive approximately $1/n$ choices.

For our next case, the more common one, *e.g.*, in rating scales, let us take numbers in a sequence, as 16, 17, 18. If, to the subjects responding, these numbers constitute a pattern in the sense that 16 is closer to 17 than to 18, the assumption of independent probability, made for our first case, is no longer tenable. The relations between the parts of this three-part whole interfere with such a simple assumption and distort any prediction of relative

frequency of choice made on that basis. But these relations are not unpredictable. A long established culture trait like a number system is learned and overlearned to the point where the behavior-evocating properties of these symbols are, in some respects at least, highly uniform. To the extent that this is true, an approach from above—*geisteswissenschaftliche* in a sense—is made possible. The culture guarantees that educated adults shall have acquired—no matter how—certain habitual responses to numbers and to relations between numbers, so that the variety of parameters, which must otherwise enter the equations to take care of variations in receptor function, motivation, opportunity, intelligence, learning methods, and the like, can be cancelled out, and our calculations thereby tremendously simplified. As a result of this long process of socialization it may be taken for granted, without further ado, that certain relations between numbers have significant effects on the behavior of educated adults: 17 is between 16 and 18, $18 - 16 = 2(18 - 17)$, etc. The uniformity of this end-product of the socialization process, regardless of the deviousness of the path to this end, is the simplification of our problem, a simplification which permits the establishment of a theoretical bridgehead immediately inside the problem area of socially significant patterns of behavior.

The chief characteristic of a number system is its great generality. When numbers such as 16, 17 and 18 are used to designate quantities of anything, one assumes that 18 refers to larger amounts of whatever is referred to by 17, and that 17 refers to larger amounts of whatever is referred to by 16. Or, to put it in behavioral language, the response to 17 will be the same in kind as the response to 18, but less in amount. Psychological theory is familiar with this phenomenon under such names as 'stimulus equivalence,' 'transfer of training,' and 'generalization.' Our problem at the moment is to express quantitatively and rationally how the behavior-evocative properties of an item in a pattern influence neighboring items in the same pattern, a problem which will recur if this kind of theoretical attack is productive.

In our case of a sequence of numbers, then, the attractiveness (= valence = behavior-evocating potentiality = . . .) of a number which is perceived as an item in a pattern of numbers is some kind of combination of its attractiveness by itself and whatever attractiveness accrues to it from its relationship to other numbers in the pattern. We shall assume, firstly, that these two kinds of attractiveness are combined by simple algebraic addition to form total attractiveness, as measured by relative frequency of choice of each number in the sequence, and secondly, that the attractiveness of each number by itself (before patterning) is the same for all; then turn to an attempt to state what fraction of an item's attractiveness spreads or transfers to its neighbors.

This task is facilitated in our special case of a pattern of numbers with definite boundaries—and perhaps in other similar patterns of abstract significance—by the existence of a break or contour between the numbers designated as inside the pattern and those outside. When instructions are given to select one of these numbers, 16, 17, 18, neither 15 nor 19 will be selected. The generalization gradient (which begins and is maximal at A, let us say) must drop to zero at the first number outside the limits of the number sequence laid down by the instructions (which number we shall call B). Using y for the height of the generalization gradient from A to B, then, and arbitrarily taking the maximum value as 1.000, $y =$

1.000 when $x = A$, and $y = 0$ when $x = B$. Anchoring both ends of the generalization gradient in this way eliminates a good deal of the uncertainty in choosing an equation to describe the gradient, and eliminates in fact any of the equations for curves which fall asymptotically to the baseline. One likely assumption is that the decline in y is proportional to some power of the distance from the starting point A:

$$y = 1 - \left(\frac{x - A}{B - A}\right)^r \qquad (1)$$

This equation states that the generalization spreads from A, the point of departure, as a function of distance therefrom. But it seems equally likely in this case that B, the point of arrival, exerts a similar influence, so that the drop in the generalization gradient is accelerated as x approaches B. The influence of B on the generalization gradient would presumably be proportional to $(B - x)^r$; hence we combine the two and introduce suitable constants so that y will continue to vary between 1.000 and 0:

$$y = 1/2\left[1 + \left(\frac{B - x}{B - A}\right)^r - \left(\frac{x - A}{B - A}\right)^r\right] \qquad (2)$$

Equation 2 represents the operation of two spheres of influence with their centers at A and B, both influences equal in strength and operating in the same direction, but located so that the high half of the gradient is largely within the influence of A and the low half largely within the influence of B.

This second equation is reasonable *a priori* and is not inconvenient to work with. The only parameter to be decided upon is the exponent r. Fortunately, when $r = 1$ or 2, this equation describes a straight line, and when other plausible values of r are chosen, such as 3, ½ and ⅓, the curve has the shape of a reversed S, departing on the whole not far from a straight line. Therefore, the assumption of a linear generalization gradient, an assumption which has worked fairly well in describing the spread within other highly generalized patterns (4, 6), is at least worthy of trial.

On this theoretical basis, then, Fig. 1 was constructed, with an initial valence of 1.000 assigned to each number in the pattern and a linear gradient drawn from this maximum value to a value of zero at the first number outside the limits set by the instructions. All that remains is to add to this initial attractiveness of 1.000 the fractions which accrue to each number in accordance with its position on the generalization gradients spreading from the other numbers. At 16 the sum of the ordinate values is 1.000 + .500 + .333 = 1.833. At 17 this sum is 1.000 + .667 + .667 = 2.333. At 18 this sum is, of course, 1.833. Adding these and dividing each by this total we get .3056, .3889 and .3056 as measures of the relative attractiveness of the first, second and third positions in a three-number scale. Similar calculations for a four-number sequence yield these fractions: .2083, .2917, .2917, .2083. And for a five-number sequence: .1522, .2244, .2467, .2244, .1522. The method is a general one which can be applied to sequences of any length.

The most prominent outcome of this generalization hypothesis can be approximated verbally by saying that the middle number of a sequence of numbers is expected to be more attractive than the end numbers because it has numbers adjacent on both sides from which it can recruit, while the end numbers are adjacent to only one. As to empirical data, the hypothesis states that, if individual peculiarities are

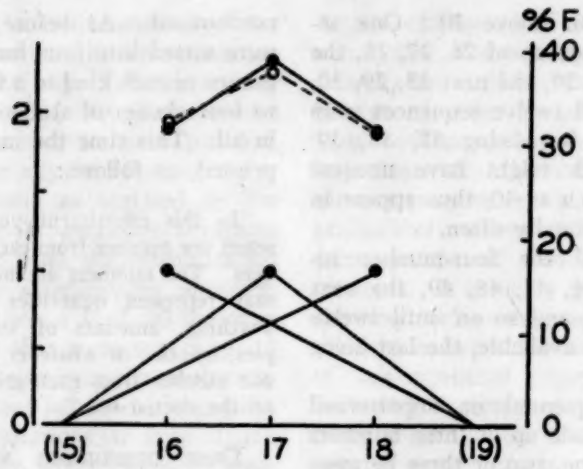

FIG. 1. Diagram to show the cumulative effect of generalization within a three-number sequence used as a rating scale. Each number by itself is assumed to have unit attractiveness (left ordinate). Generalization gradients descend from these origins to zero at the first number outside the sequence. Each circle at the top represents the sum of the ordinate values at each number, from which relative frequencies of choice of numbers are calculated (right ordinate). Open circles represent empirical data from 981 sequences.

smoothed out by the use of a variety of sequences and a large sample of subjects, these fractions are the relative frequencies to be expected for the numbers in each position in the sequence. Since these relative frequencies permit rather precise prediction, with no constants to be assumed for the empirical data, and no fudging on the fit of a curve, one turns eagerly to an experimental trial. Several tests of the theory have been carried out, and they are all alike in that several number series were printed on paper and given to college students with instructions to pick a number from each. They differ slightly in method and in results; hence they can best be described chronologically.

In all of these experiments the numbers have been presented to the students merely as numbers, not as numbers of oranges, or admirals, or amounts of annoyance. It is altogether possible that the stimulus properties of '16 oranges' are more similar to the stimulus properties of '15 oranges' than those of '16' are to those of '15,' and that the effects of the generalization gradient would consequently show up more significantly in the calculations. But it seemed likely that giving the numbers some specific reference would increase the weight of idiosyncrasies of choice and thus necessitate more careful balancing of the sequences. Therefore instructions were given to regard the numbers as unspecified magnitudes.

THE FIRST EXPERIMENT

In the first experiment several three-number and four-number sequences were used and, in addition, groups of three numbers which were not in sequence, so that patterned groups could be compared with unpatterned. To avoid lucky and unlucky numbers the

sequences began above 20. One sequence was made up of 26, 27, 28, the next of 27, 28, 29, the next 28, 29, 30, and so on until twelve sequences were available, the last being 37, 38, 39. Numbers which might have unusual significance, such as 30, thus appear in each position equally often.

The first of the four-number sequences was 46, 47, 48, 49, the next 47, 48, 49, 50, and so on until twelve sequences were available, the last being 63, 64, 65, 66.

The non-sequential or unpatterned groups were made up of three numbers with gaps of one, two or three between adjacent numbers. The problem of balancing is not difficult in this case. Examples are: 41, 44, 46 and 52, 54, 57. Twelve groups were so constructed.

We now have three kinds of number groups, twelve of each. Since four classes of students were available at the time,[1] these twelve groups were sorted into four forms of three each as evenly as possible. Each student was therefore presented with a set of nine number groups, three of each kind, with oral instructions to pick one number from each group and enter it on the dotted line in front. Time required was about five minutes.

The results from these 204 groups of numbers (3 × 68 students) of each kind were promising in a preliminary way, justifying another trial.

THE SECOND EXPERIMENT

In the second experimental test the same number groups were used as before but, to improve the balancing, half were printed in ascending order on the page, and half in descending order. Also, the order in which the three kinds of groups appeared on the page was randomized. As before, these groups were sorted into four forms, with three groups of each kind to a form, and given to four classes of students, 94 students in all. This time the instructions were printed, as follows:

"In this experiment you are asked to select one number from each group of numbers. The numbers are not specific; they may represent quantities or amounts of anything: amounts of water, anger, apples, months, or whatever you wish. Pick one number from each group and write it on the dotted line."

These precautions were taken on *a priori* grounds; there was nothing in the results of the first experiment to indicate bias. And the results of the second experiment were not noticeably different from those of the first. The two are therefore combined and presented in Table 1. The number of groups, of each of the three kinds, presented was 486 (3 × 162 students).

TABLE 1

COMPARISON OF PREDICTED AND OBTAINED FREQUENCIES IN THE FIRST TWO EXPERIMENTS

					χ^2	P
Three-number sequence						
Predicted	148.5	189	148.5			
Obtained	149	185	152			
					.18	.90
Three separated numbers						
Predicted	162	162	162			
Obtained	164	160	162			
					.06	.95
Four-number sequence						
Predicted	101	142	142	101		
Obtained	121	141	145	79		
					8.86	.03

RESULTS OF THE FIRST TWO EXPERIMENTS

In these tables the first entry on the left indicates the frequency of choice of numbers in the first position on the left as the sequence of numbers is read in the usual way. The other entries

[1] The writer is indebted to all teachers of Psychology 1 at the University of Illinois during the Summer and Fall of 1945 for making their classes available.

likewise refer to frequencies of choice of the numbers in the corresponding positions on the page. If the balancing has been adequate, these entries show the preference for each position, rather than for any number *per se*, or for high numbers as opposed to low ones. For the three-number sequences the predictions from the generalization hypothesis are astonishingly accurate. In this comparison a P of .90 indicates the probability that a chi-square of .18 or more could arise from chance fluctuations. Such close agreement is gratifying, but one questions at once if any other hypothesis would work as well. There is no other familiar hypothesis which could be applied to this situation except the hypothesis that each position would be used equally often. Applied to these data such a hypothesis would give frequencies of 162, 162, 162. Testing this in the usual way we get a chi-square of 4.9, and a P of .10. Therefore, although the hypothesis of random choice leads to far greater discrepancies from fact than the generalization hypothesis, it is still not completely disproved.

When the three numbers are separated by a gap of one or more numbers, one would expect that the pattern is broken up or, in the sense of the theory presented here, that the generalization gradient drops to zero at each gap (since the numbers ordinarily occupying these locations are outside the limits of the instructions). If this is true, no generalization occurs, and the assumption of independent probability of choice is a good one. In this case, then, the obtained frequencies should be approximately equal, and Table 1 shows that they were, without doubt. It is still possible—reversing the argument of the above paragraph—that the frequencies predicted from the generalization theory would fit these data. Testing this assumption we come out with a chi-square of 7.1 and a P of about .03. The assumption is not disproved, but it is at least questionable. We are justified in concluding, with high confidence, that the generalization hypothesis fits the data from three-number sequences and that the hypothesis of random choice fits the data from groups of three non-sequential numbers. It is highly questionable that the frequencies computed from the generalization hypothesis will fit the data of non-sequential groups or that the frequencies computed from the hypothesis of random choice will fit the data of patterned groups. So far, the theoretical predictions are nicely verified.

Turning to the bottom third of Table 1 we see that for the four-number sequences the theory meets a setback. The trend of the data is in the direction required by the theory but not far enough to reach the .05 level of probability. The alternative hypothesis of random choice fares even worse (chi-square = 12.5; $P < .01$), partly because of the same asymmetry which throws the generalization theory off the mark.

THE THIRD EXPERIMENT

The third experiment extended the procedure of the first two: a larger variety of number sequences, even more carefully balanced, a larger number of subjects, and the inclusion of five-number sequences. (No separated number groups were included as the random nature of the choices in that case was considered fairly well established in the first two experiments.)

Eighteen three-number sequences, twenty-four four-number sequences and thirty five-number sequences were constructed, half of which were printed in ascending order of magnitude, half in descending order. Multiples of ten were avoided; multiples of five appeared in each position equally often.

These sequences were then randomized and sorted into twelve forms, so that Forms A, C, E...K consisted of two three-number sequences, two four-number sequences and two five-number sequences, while Forms B, D, F...L consisted of one three-number sequence, two four-number sequences and three five-number sequences. The twelve forms were then scrutinized to eliminate any peculiarities which might favor any one position. Instructions were printed as above, with the addition of one line: "Before writing each number think how large it is." These were presented to twelve classes of students, ranging in size from 21 to 32, with a total of 327 students (after two papers were discarded). This gives us for analysis 495 three-number sequences, 654 four-number sequences, and 813 five-number sequences.

As may be seen in Table 2 the numbers picked from the three-number sequences again come very close to the prediction. To clinch the case for the three-number sequences we can combine the frequencies for these 495 sequences with the previous 486, of table 1, and compute a chi-square from the data of all 981. This chi-square comes out as .39, and P would be above .80. Such results from a sample of this size demonstrate rather convincingly that the relative frequencies predicted from the generalization hypothesis fit the facts very well indeed. Furthermore, the hypothesis of random choice is definitely ruled out when all 981 series are tested (chi-square = 9.3; $P < .01$). Figure 1 shows this comparison graphically.

For the four-number series the prediction is quite wide of the mark. If we combine these figures with those from the four-number sequences shown in Table 1, some of the irregularities are evened out, and chi-square is reduced to 25.5, but that still not does reach the

TABLE 2

COMPARISON OF PREDICTED AND OBTAINED FREQUENCIES IN THE THIRD EXPERIMENT

		x^2	P
Three-number sequence			
Predicted	151 193 151		
Obtained	158 187 150	.52	.80
Four-number sequence			
Predicted	136 191 191 136		
Obtained	169 149 160 176	34.07	<.01
Five-number sequence			
Predicted	124 182 201 182 124		
Obtained	167 148 174 133 191	73.90	<.01

.01 level of probability. The alternative explanations are: (1) that the generalization which seemed to be so convincingly demonstrated in the three-number sequences does not operate in four-number sequences, or (2) that some other tendency comes into operation to raise the frequency of choice of the first and last numbers and depress the second and third. These alternatives will be taken up again later.

Turning to the five-number series we find the same elevation of the ends. Numbers in the first and last positions are high; those in the second and fourth are low. These results are interesting for their own sake, in addition to their value as a check on the theory. These frequencies are not the result of random choice, for calculation of chi-square on that basis comes to 13.17, which is just below the .01 level. There is a regularity in these frequencies which is worthy of further study.

THE FOURTH EXPERIMENT

This experiment concentrated on the five-number series in the hope that the results would show more clear-cut trends, uncomplicated by the presence on the same page of three-number and four-number series. And, since the end-effect noted in the previous ex-

periment pointed to the importance of perceptual factors, such as the arrangement of the numbers on the page, half of the series in this experiment were printed horizontally, with a dotted line in front, as in all previous experiments, and half were printed in vertical order, with the dotted line underneath. The thirty five-number sequences, half ascending and half descending, used in the third experiment were sorted into five forms of six sequences each and printed horizontally to make Forms M to Q. Then they were printed vertically to make Forms R to V. These ten forms were presented to ten classes of students, ranging in size from 23 to 32, making a total of 792 horizontal sequences (6 × 132 subjects) and 900 vertical sequences (6 × 150 subjects).

TABLE 3

COMPARISON OF PREDICTED AND OBTAINED FREQUENCIES IN THE FOURTH EXPERIMENT

						χ^2	P
Horizontal							
Predicted	120	178	195	178	120		
Obtained	161	127	188	141	175		
						61.75	<.01
Vertical							
Predicted	137	202	222	202	137		
Obtained	167	177	222	163	171		
						25.64	<.01

The results of the fourth experiment, as set out in Table 3, are similar to the results for the five-number series of Table 2. Frequency of choice for the middle number of each series is high, approximately as predicted by theory, but the end numbers are much higher than expected and the numbers in second and fourth positions correspondingly lower. This end-effect, as it might be called, is much less noticeable when the numbers are printed vertically, and the discrepancies from theory that much smaller, but chi-square is still beyond the .01 level. Since it can be shown, by combining the two sets of obtained frequencies and applying the chi-square test to the 1692 series, that these frequencies are not the result of random choice, we must next ask what is responsible for this more-or-less cyclical distribution of frequencies which has appeared now in three sets of data obtained from three large samples, each under somewhat different conditions.

ALTERNATIVE INTERPRETATIONS OF THE DATA

It is obvious that the neat little generalization theory, which accounted so nicely for the data of the three-number sequences, does not work for the five-number sequences. Calculation of the spread of attractiveness may, to be sure, be made on the basis of some other equation than the linear one chosen. But generalization along a gradient of any plausible shape would necessarily yield frequencies for the second and fourth positions which are higher than those for the first and last. Hence we must either reject the generalization hypothesis or amend it to bring it into line with the facts. Several types of analysis were undertaken, therefore, in an effort to disclose the principles underlying the obtained data.

1. *Correction for end-effect.* The generalization hypothesis is in accordance with the fact that the frequency of the middle number in the five-number sequences is generally high, constituting the largest consistent deviation from random frequencies. The discrepancies between the generalization theory and the obtained facts are at both ends of the scale, as if the great prominence of the first and last positions pulled a portion of the choices from the second and fourth. Indeed one can assume that each end position attracts a certain fraction of the choices from the adjacent positions, the second and fourth, make the appropriate corrections in the frequencies required by the

TABLE 4

COMPARISON OF COMPUTED AND OBTAINED FREQUENCIES FROM ALL FIVE-NUMBER SEQUENCES

						x^2	P
Predicted	381	562	619	562	381		
End-correction	127	−127		−127	127		
	508	435	619	435	508		
Obtained	495	452	584	437	537		
						4.63	.30

generalization hypothesis, and fit the facts quite well. Table 4 shows what happens when all five-number sequences are combined—2505 in all—and the predictions from the generalization theory are corrected by assuming that 5 per cent of the choices migrate to the end positions from the next adjacent positions. (Also see Fig. 2.) But the end-effect is stronger for the horizontal series that for the vertical series—as one would expect if it is in part a perceptual phenomenon. Treating the three sets of five-number data separately we find that the best approximations to the obtained data, according to a least-squares technique, are computed when the correction factor is 6 per cent for the 813 series of Table 2 (chi-square = 8.08; $P = .10$), 5.8 per cent for the 792 horizontal series of Table 3 (chi-square = 1.73; $P = .80$), and only 3.6 per cent for the 900 vertical series of Table 3 (chi-square = .62; $P = .95$). For our 1140 four-number sequences, from Tables 1 and 2, the end-effect is large, a correction of 7.7 per cent giving the best fit (chi-square = 2.68; $P = .50$). In the case of the

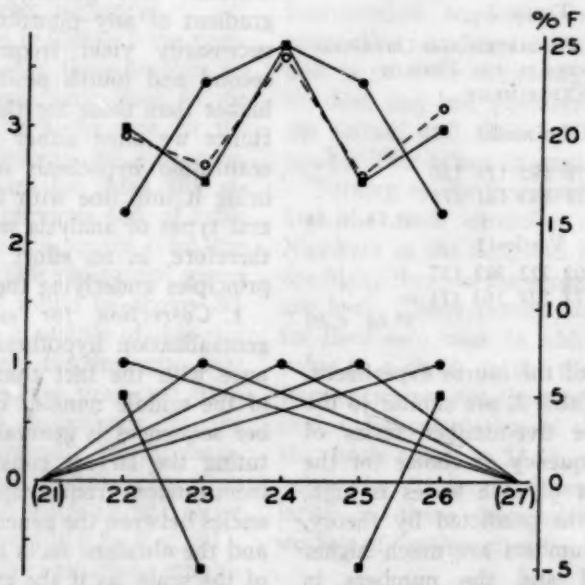

FIG. 2. Diagram to show intra-scalar effects when a sequence of five numbers is used as a rating scale. As in Fig. 1 the filled circles at the bottom and the gradients descending from them represent generalization, the effects of which at each ordinate are cumulated to obtain the values represented by the filled circles at the top. The squares are intended to indicate a 5 per cent end-effect which is combined with the generalization principle to get the W-pattern of frequencies shown with squares at the top. Open circles represent empirical data from 2505 sequences.

three-number series one can say simply that the end-effect does not occur because there are no numbers between the ends and the middle. Or one can suppose that the ends recruit choices from the next adjacent number, which would subject the middle position to piracy from both sides. The optimum fraction for the end-effect computed in this way for all three-number series is, of course, small, only 0.5 per cent (chi-square = .045; $P > .95$). Evidently predictions from the generalization theory plus empirical corrections for the end-effect describe the facts quite well.

2. *Cyclical analysis.* From another point of view one is impressed, when examining the obtained data for the five-number sequences at least, by the periodic nature of the frequencies: first position high, second low, third high, fourth low, fifth high. Such periodicity in psychological data is very interesting and merits an effort at analysis. A general expression for a trend of this nature which would apply to sequences of any length can be written by converting the length of the number sequence to two revolutions of a circle, or 720°, and using a sine function. Letting x stand for the position of a number in a scale, running from 1 to n, an equation, convenient for computation, can be put together, as follows:

$$F_x = \frac{1}{n} + k \sin\left[90° + \frac{720°}{n-1}(x-1)\right] \quad (3)$$

Thus we can get frequencies of positive and negative sign from the second term in this equation to be added to the $1/n$ frequencies expected from random choice.

The difficulty in the use of this equation, of course, is the constant, k, representing the weight to be given the cyclical principle. For the data of all five-number sequences combined, as in Table 4, the best-fitting value of k is about .02, and computations on this basis lead to frequencies of 541, 441, 541, 441, 541 and to a chi-square of 8.27 and a P a little above the .05 level. Much better fit can be attained by taking the two horizontal five-number sequences separately and assuming weights for k of .03. For the vertical five-number sequence, however, the best fit is obtained with $k = .01$ but the fit is barely above chance expectations. For the four-number series of Table 2, with $k = .02$ an excellent fit is obtained, but for the four-number series of Table 1 such computations lead to absurd results. For the three-number sequences the cyclical principle changes only slightly the frequencies expected from random choice, a pattern which has already been ruled out. Hence the applicability of the cyclical principle is definitely limited, the chief reason being that it gives equal frequencies for the middle and the ends, whereas we have seen that the middle position in the scales of three and five numbers is generally high.

3. *Combining the generalization and cyclical principles.* Several plausible ways of combining these two principles, with various values of k, were tested by the chi-square test, and most of them were quickly eliminated. This in itself is encouraging. With at most only four degrees of freedom one might expect that the data could be well approximated by a fortuitous selection of parameters. But the pattern of frequencies is quite definite, and a fit is not easily obtained. The best fit is obtained when generalization is assumed to follow a square-root law, that is to say, the spread of attractiveness is computed by Equation 1, with $r = \frac{1}{2}$, and the frequencies computed in this way are combined with those computed by the last term in Equation 3. The results computed in this way are shown in Table 5 for comparison with the obtained fre-

quencies. The table shows that a combination of these principles suitably weighted will account quite well for the facts of the case. When the different sets of data are tested separately, it is apparent that the only reason why the data cannot be matched with negligible disparity is the asymmetry which occurs in some distributions, *e.g.*, the four-number sequences in Table 1, and the five-number sequences in Table 2. For the other four-number sequences (see Table 2) taking k as .06 leads to a chi-square of only .93 and a P of about .80. For the other five-number sequences (see Table 3) the best value of k is .04 for the horizontal series (chi-square = 3.15; $P = .60$) and .025 for the vertical series (chi-square = .64; $P = .95$). For the three-number sequences the cyclical principle yields equal frequencies at all three positions; hence its effect is only to modify the frequencies predicted by the generalization hypothesis in the direction of random choice. For small values of k this effect is not serious, as Table 5 demonstrates.

TABLE 5

COMPARISON OF OBTAINED FREQUENCIES (ALL SERIES) WITH FREQUENCIES COMPUTED BY COMBINING GENERALIZATION AND CYCLICAL PRINCIPLES

						χ^2	P
All three-number sequences							
Obtained		307	372	302			
Computed ($k=0$)		311	359	311			
						.78	.70
($k=.05$)		313	355	313			
						1.32	.50
All four-number sequences							
Obtained	290	290	305	255			
Computed ($k=.01$)	269	301	301	269			
						2.68	.50
All five-number sequences							
Obtained	495	452	584	437	537		
Computed ($k=.03$)	490	449	626	449	490		
						7.81	.10

EVALUATION AND DISCUSSION

The generalization hypothesis seems to be useful for the analysis of these results, but it has to be supplemented by some other principle. Of the two additional principles discussed, the end-effect and the cyclical trend, the former has the better rationale and could perhaps have been expected from careful examination of published findings from rating-scale studies. The generalization principle is a stable one, elevating the middle frequencies under nearly all conditions, while the other principles discussed appear irregularly and vary considerably in weight.

This end-effect, which is of considerable interest in its own right, is not a new concept in psychology by any means. Studies of tachistoscopic perception of letter groups (see Woodworth's [9] chapter on reading, for instance), many experimental investigations by Gestalt psychologists emphasizing the observer's preoccupation with the contour separating figure from ground, which in the present case would be the end numbers, and more recent analytical studies of the anchoring of rating scales, most of which have been summarized by Johnson (5, p. 210), all point to the special importance of the end positions. It would be highly desirable to know more about this end-effect. In a sequence of ten numbers, for example, how far toward the middle would the ends extend their influence? Assuming a gradient from the ends inward, we have the same problem of deciding upon the shape of this gradient—quite arbitrarily taken as linear in Fig. 2—that we have discussed earlier for the generalization gradient extending from all numbers. Ideally, of course, the peaks at each end would be described by the same theory which describes the peak in the middle.

Coming back to our data we can say that, when the results are broken down

into a generalization principle and an end-effect, the end-effect is closely dependent upon perceptual arrangements, being much greater when the sequences are printed horizontally than when they are printed vertically. Since the end-effect bulks rather large in some sets of data, could the end-effect alone account for the obtained frequencies? It is only necessary to examine the foregoing tables to answer this question. The end-effect could account for the four-number data in Table 2, but not for any other sets of data.

An important technical problem which has been slurred over in this paper is the way in which different valences or potentials are combined. In computing total attractiveness we have merely added the separate increments which accrue from all sources. Why not multiply? or take a mean of some sort? Faced with a similar problem in combining habit strengths Hull (2, pp. 199–203) uses an equation by Day which makes the contribution of each item smaller as the total approaches a maximum. Other kinds of interaction have been discussed by the writer (5, pp. 200–207) for the case where several heterogeneous factors enter simultaneously into the determination of an overall or summary judgment. The question of mode of summation is not crucial at the moment, however, because our sums are all divided by the same total and converted into relative frequencies. Many different modes of summation would yield the same results.

SIGNIFICANCE OF THE DATA FOR THE USE OF RATING SCALES

Regardless of the theoretical analysis put forth in the above paragraphs the empirical data have some practical bearing on the interpretation of results from the use of rating scales. The frequencies shown in Table 5 are not the results of random choice; in scales of three, four, and five points one cannot assume —as is occasionally done—that the positions are equally attractive to begin with, and that any deviation from $1/n$ frequencies is an indication of the influence of the material being rated. It would be better, judging from the variability of the results from many samples of about two dozen each, not to make any assumptions about expected frequencies, but if assumptions are made, the frequencies shown in Table 5 have a better empirical basis than the assumption of random choice, or $1/n$ frequencies in each position. In the three-point scale our data show relatively little variation; the frequencies shown in these tables appear quite regularly, even with small samples. The four-number series show considerable variation, as one can see by comparing the frequencies in Tables 1 and 2, so perhaps it is well that the four-point rating scale is not widely used. As to the five-point scale, which is widely used in ratings, it can be shown by the chi-square test that our three sets of data, obtained under three slightly different conditions, could be samples from the same distribution. Within this distribution the greatest variability, in the sense of the largest contribution to chi-square, comes from the last number in the series, Position 5. Position 2 is next. The most stable positions are the first and fourth. With large samples one can expect about 20 per cent of the judgments to land in the first position, 22 to 24 per cent in the middle position, and about 17 or 18 per cent in the fourth position. The remainder will fluctuate between the second and last positions.

Two other empirical findings are of some interest. Since multiples of five appeared in each position equally often, their popularity, independent of position, can be easily determined by a simple tabulation. In the 2505 five-

number sequences these numbers received 21.6 per cent of the choices, only slightly more than the expected 20 per cent. Since the sequences were printed in both ascending and descending order of magnitude, it can be easily determined whether there is a preference, independent of position, for high, as opposed to low, numbers. In the same 2505 sequences the highest number in the sequence received 92 more choices than the lowest, slightly less than 4 per cent of the whole number of choices. In contrast, it can be seen from Table 5 that the preference for the last number on the page over the first, independent of magnitude, was slightly less than 2 per cent of the whole. These preferences are all of small strength. It is interesting to contemplate what happens when a favored number, which is the highest of the series, occupies a favored position. An analysis of the data at hand from this point of view is in progress. Also, an investigation of individual differences in preference for the extreme positions of the scale, rather than the middle, is planned.

Of course, when the numbers of a rating scale are given specific meaning, as in referring to amount of emotionality or excellence of performance, the weight exercised by intra-scalar, as opposed to extra-scalar, relations is another matter. In this case one would expect the generalization to be stronger and more stable, but empirical data are needed. Careful researchers have always avoided making any critical assumptions about rating scales. Usually the experiment can be set up so that the conclusions come from the same rating scale when used under different conditions or by different samples.

SUMMARY

This paper approaches the problem of theoretical analysis of behavior in relation to *patterns* of stimuli by a frontal attack on the most completely generalized and abstract pattern of our culture, namely, the number system. Since the socialization process guarantees that educated adults possess certain well-known uniform habits of response to numbers and number relations, our equations can eliminate the usual troublesome parameters for sense organ function, amount and direction of reinforcement, ability to learn, and the like.

The problem is set up as a study of responsiveness to short sequences of numbers arranged as in rating scales, *e.g.*, 16, 17, 18, except that the numbers have no reference to specific objects or attributes. It is assumed that the attractiveness (or valence, or behavior-evocation properties) of a number, as indicated by frequency of choice of that number, can be treated as the sum of the attractiveness of that number by itself—which would be the same for all numbers and would equal $1/n$—and whatever additional attractiveness generalizes or spreads to that number from neighboring numbers by virtue of its position in the scale. The form of the generalization gradient thus becomes an important question, which is examined both rationally and empirically. Calculations on the basis of any plausible assumption about the generalization gradient lead to an expectation of higher frequencies for the middle position of the scale. With the assumptions made in this paper calculations for a three-number sequence yield relative frequencies of .3056, .3889, .3056.

Number sequences of three, four and five numbers were made up and carefully varied and balanced in order to test the predictive power of the generalization hypothesis. For groups of three numbers in a sequential pattern the theory works very well indeed.

For non-patterned groups of three numbers the prediction is that random choice will prevail, and this prediction also is verified. Thus considerable support is given to this method of analysis.

For the four-number and five-number sequences the theory falls quite wide of the mark in most cases. Further analysis shows that the data from all sequences, 4590 in all, can be successfully treated as a combination of the generalization principle and either an end-effect or a cyclical trend.

Aside from the theoretical treatment the results from this large number of responses show that, regardless of what is being rated, the various positions of a rating scale are not used with equal frequency. The scale is loaded. Certain positions have special attractiveness, and Table 5 shows what relative frequencies to expect under conditions similar to those used here.

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